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ON THE DEVELOPMENT OF VOLUNTARY MOTOR ABILITY.

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PREFACE

On the Requirements of Work in Experimental Psychology.

I.

Work in Experimental Psychology must meet two requirements. It must be carried out according to the best attained methods of scientific research; and its results must contribute something to the knowledge of conscious life. The latter requirement is sometimes expanded to mean that the contribution must throw effective light upon general problems of Psychology and Philosophy; and sometimes it is expanded to mean that the contribution must be of some practical use.

II.

The Experimental Psychologist has no choice and no wish for choice against the requirement for exact method. It is true, indeed, and needs emphasis, that in the present state of Psychology a vast deal is to be hoped in certain fields from very simple methods, intelligently planned. More exactness than the subject requires is pedantry and waste. It is a fundamental error to suppose that the same exactness in experiment, and the same strictness in deduction, are possible or useful in all scientific work. It is a principle of wide application that degree of system in procedure should correspond to the degree of system in the material and situation

to be dealt with. To establish in a logging camp, the governmental machinery which is found essential at Washington, to keep debit and credit for a kitchen garden with the system of books used in the New York Clearing House, to enforce at a fox-drive the discipline of the German army, would be ludicrous violations of the principle. But a multitude of examples no less ludicrous appear in the history of science. Systematic methodology has, hitherto, almost wholly failed to recognize that science is subject to the law of evolution,—*exists* in all degrees of development. The constantly recurring delusion has been, that processes found fruitful in the more highly developed and exact fields of science, may profitably be applied to all phenomena whatever. As a matter of fact, the "inexact sciences" are only burdened by "scholastische Zahlenhaufen" (Münsterberg's apt word), which are more precise than significant, or by strict deductions, whose strictness can be only in words. We need a logic based upon the historical development of science to set forth the whole law in this matter. Meanwhile the individual investigator must be a law unto himself.

At the same time it must not be forgotten that Physiological Psychology is not so new as its name. It is historically a special outgrowth of older sciences. Its oldest classics are the work of men trained in Physiological research. The modern Psychologist, accordingly, fails to find in his specialty a paradise of windfalls. He acquires, instead, a practical realization of Comte's generalization that the more complex science presupposes and works by means of the other sciences. If there were any such thing as a perfectly trained Experimental Psychologist, he must have had thorough apprenticeship not only in the results, but also in the technique of Chemistry, Physics, Morphology, Physiology and Mathematics. Or since no one man can have all these knowledges and skills, the body of Psychologists must have them. The combined results of human ingenuity in every direction may and must be made to bear upon the elusive problems of conscious life. Such concentration of all available forces upon the problem in hand is the ideal of every Psychological research. To do this, at least in the measure attained by works of standard excellence in the same field, is a requirement not to be escaped.

III.

The demand for results of general significance requires consideration. Perhaps no one has ever been more urgently called upon to consider this demand than the Experimental Psychologist. For, perhaps no one has been more urgently

solicited from within and from without to "keep his feet upon the earth and yet to carry his head among the stars." He is called upon to be both scientist and philosopher in one. As a matter of practical attitude, at any rate, every Psychologist must give the demand for general results some kind of answer.

This demand seems to be eminently rational. For progressive organization seems to be a universal character of growth. All evolution, organic and inorganic, conscious and unconscious, individual and racial, appears to involve advance of the relatively isolated into more perfect unity. The main occupation of every living thing seems to be the transformation of a relatively chaotic environment into stuff of finer organization; and degree of organizing capacity seems to be one fair measure of a being's place in nature.

One who has arrived at this conclusion, with whatever arguments therefor, is rarely undecided in his judgment of the relative value of concrete and general scientific results. The concrete scientific result, standing in no obvious relation to any general law, is not, he may allow, without value; but its value diminishes the more concrete and isolated it is. In the presence of the greater generalizations of science, such unrationalized bits of knowledge seem to him trifling; in the presence of the insights of philosophy, they become practically insignificant. From men who have this view, the demand upon Experimental Psychology for general results is particularly imperative. "Do your experiments throw light upon the nature of the soul?" they ask. "Do your statistics determine a system of Psychology? Does your delicate machine enable you to establish, or disestablish, any general law of mind? If not, if you have nothing to show but an uncorrelated fragment of information about the conscious life of infusoria, or about the knee jerk, or about the time in which you can wag your finger, all this may be very exact, but it is almost impertinent to call it a contribution to Psychology and it is altogether folly to call it a contribution to Philosophy."

What answer can be made?

1. In the first place, it is submitted, subject to the facts of the History of Science and Philosophy, that reaction against established general theories toward concrete reinvestigation from the ground up, has justified itself as an essential part of the process of intellectual growth. "Jeder tüchtiger Denker ist zuerst Zweifler," says Herbart. One must have studied the lives of the most productive men of history very superficially indeed not to see that this is so,—that the resolute skepticism, negation and descent which, for example, Des

Cartes describes in himself, must prepare the way for the more obviously profitable creative work.

The same thing appears no less in social Psychology. Mr. William T. Harris has observed, as one of the mysterious phases of the History of Philosophy, that the "shallow thought of Nominalism should have triumphed for a long period over the deeper and truer thought" of St. Thomas Aquinas and the orthodox theology. He offers in explanation of this mystery the consideration that the "deeper and truer thought," although it "emancipates humanity at first, after a time imposes on the soul a sort of external authority and needs to be replaced by a newer freedom." . . . . . "It is wonderful," he says, "to see how the most negative phases, the skepticisms, the heretical doctrines, the most revolutionary phases in history . . . . are destructive only in their undeveloped state and when partially understood. By and by they are drawn within the great positive movement, and we see how useful they are become."

Suppose now the results of Psychological research were as fragmentary and unmeaning as the most ignorant critic believes or hopes, how should we account historically for the social movement which has produced them? How could men who sat at the feet of Kant, Hegel, Herbart, and other such masters, turn away to these husks?

Might we not make a beginning of answer with the foregoing explanation of spiritual revolt? Might we not say that the modern movement in Psychology constitutes a protest against the final sufficiency of the howsoever superior systems which the world already possessed? If this movement were wholly negative, heretical and destructive, might we not expect that it would "by and by be drawn within the great positive movement" toward a philosophy, through it more rational and emancipating than those it forsakes.

No such revolt is nihilistic. It is essentially an appeal from the schematized reason of the books to the finer reason of reality. It is an expression of that saving discontent which drives men always from the good toward the everlasting better. To regard such a movement as a descent is an altogether distorted view. If it be descent, it is the descent in search for solider foundations. If we dig down, it is that we may build the higher.

2. We have, however, a far more fundamental justification of concrete Psychological research when we view it as —what it is intended to be—a contribution to a long cooperative task. This way of looking at individual work is very familiar in Philosophy and Higher Anthropology. From many sources ancient and modern, scientific and phil-

osophic, from Aristotle, Leibnitz, Hegel, Darwin and Spencer, we have some more or less explicit statement of this view. Human life in this world is regarded as a development, to which every man's individual accomplishment is a more or less important contribution. Therewith, conscious activities and attainments—whether of child, savage, average civilized man, poet, saint or philosopher—are made to appear as stadia in the growth of mind. We have, accordingly, at bottom the view of *a race working together*, consciously and unconsciously, by force of circumstances, by instinct, or by intelligent purpose, through the long task of comprehending the world.

This view carries with it two direct implications. The first is that *no* sort of intellectual effort is quite without justification. The ideas of childhood and of the childhood of our race, the myths, cosmogonies, and grotesque theologies, as well as the scientist's fact and the philosopher's generalization—who shall say that any one of these has contributed nothing to the development of culture? To despise the study of the conscious life of a spider is unphilosophic. To despise the study of the conscious life of Plato is unscientific. To be dismayed at the world full of warring ideas as though they threatened the Sovereignty of Reason is unintelligence and lack of faith. These derisions and fears are no doubt natural—to minds of a certain development—but they disappear from every mature and reverent view of the world. The second implication of this view is that intellectual values are not equal. It is not a theory of indifferentism. On the contrary, growing out of this view, or in essential harmony with it, is a universally recognized standard for valuing intellectual work, namely, how much has the work contributed toward accomplishing the intellectual task of the race?

Now it is a singular fact that while a philosopher is much the more apt to recognize this view in theory, a scientist is much the more apt to realize it in practice. The philosopher is very much inclined to limit the application of the theory to his predecessors and contemporaries. The instinct of the philosopher is to complete by himself, in the general, the intellectual task of the race. Very often he believes he has done so. In his system, the long intellectval evolution has culminated.[1]

[1]For illustration of this tendency in the philosopher, one thinks perhaps most readily of Hegel, whose exposition of the History of Philosophy as a necessary evolution, wherein individual systems are necessary successive stadia, is not felt by him to be inconsistent with the claim that his own system is a culmination of the evolution, a finally valid general view of all reality.

A not less interesting illustration of the same tendency is to be found

Quite otherwise the scientist. The Experimental Psychologist, for example, may or may not have a Philosophy of History wherein every man's work, including his own, is regarded as a small contribution to a social task. He may not have the theory, but he does the thing. From inclination or from resolution he foregoes the making of a system, and tries to furnish some material for one. He is cheerfully willing to fix one point and drive a peg down there, whether any other peg is in sight or not. He makes no apology for his uncorrelated fact. He denies the right of the present to determine its final value. He leaves it for the justification which time shall show. He is willing, in short, to make one in a vast social endeavor instead of trying to complete the whole task by himself.

The foregoing is intended to be a defense of concrete Psychological research, even when the results throw no immediate light upon general theories of life and mind. It has been for the time conceded that the results in Experimental Psychology are all of this character. The concession was, however, only temporary. It is flat ignorance to suppose that the body of Psychologists are working without intelligent aims, somewhat, for example, as the earth-worms, which contribute to civilization without intending to do so. It is true, indeed, that there are men in this as in other fields of science who profess horror of generalizations. When the History of Science is written from the Psychological standpoint, the etiology and uses of this type will no doubt be made to appear. It may be that those often skilful and productive scientists, whose fear of generalizations amounts to a phobia, represent the extreme swing of the pendulum from the other extreme of reckless speculation. They perhaps exhibit, in the social scientific movement, in a large and obvious way, that period of skepticism, negation and blind groping which the individual thinker generally passes through on his way to a more obviously productive period.

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in that present day philosopher who is popularly supposed to be the special champion of empirical science and the special foe of dogmatic philosophy. Mr. Herbert Spencer presents a system of First Principles, of which it is affirmed:

1. That Mr. Spencer is the first in the course of evolution fully to realize them. 2. That they are strictly deducible from an ultimate principle, which permits empirical illustration, but which does not permit empirical proof. 3. That they hold good for the whole and for every part of every one of an infinite number of successive epochs of world-evolution and dissolution. How it can be so confidently foreknown that the Unknowable—in which all things, changes and laws have their being—contains no potential modification of Mr. Spencer's valuable generalization, is not known to the writer.

Such periods are probably necessary to the philosopher, and such men are useful, negatively and positively, in the development of science, even if they generally are as they have been aptly called, "die Handwerker der Wissenschaft."

The leaders in Psychological research are, however, not at all of this description. The ignorant derision and neglect which so often express the attitude of the immature mind toward philosophy, find no sympathy from any recognized master in modern Psychology. It is, on the contrary, a fact that the leaders in this movement are not only acknowledged masters in experimental science but thoroughly schooled in philosophic disciplines. Munk has expressed the ideal in speaking of Helmholtz: "*Bei der genauesten Ermittelung des Einzelnen, das Ganze nicht aus dem Auge zu verliern; bei der Erwägung des Allgemeinen immer wieder Kraft und Sicherheit am Besonderen zu erproben*" (1). It is an inestimable good fortune that this is so. For a social movement, howsoever much it may be beyond the precise control of any man, is nevertheless even as other force, largely directed by the men who best understand it.

Under such leadership, the rapidly growing company of Psychologists have learned to take no narrow view of their common task. That study of the development of the unconscious world whose results make up most of what goes under the name of modern science, Psychology will supplement by a study of the development of conscious life, from its darkest beginnings to its apotheosis in science, philosophy and religion. This work, it is profoundly believed, cannot be forestalled by general laws, however obtained and however true. We cannot dispense with organized mortality statistics, because we know that all men are mortal. The most ingenious philosophic reflection cannot anticipate the special phenomena of human activity and the special laws which they will reveal. There is no device for avoiding the task which the actual, finite and definable interactions between individuals and society make possible and imperative. "Die Höhe reizt uns, nicht die Stufen" Gœthe's Wilhelm Meister is told. But the *Stufen* are not to be escaped. It is, therefore, evident that this must be, in the broadest meaning, a co-operative task. Money from men of wealth, or from all the people through the state; the experience and manual skill attained in the mechanic arts; the instruments, devices and discoveries of the older sciences; the suggestions of Philosophy back to the earliest myths; the divinations of art and religion; men able to plan, and men willing to work; all the forces in co-operative civilization must come together for the making of the Science of Psychology.

IV.

The demand for practical results, which has every meaning that varying stages of culture give to the word practical, has already received an implicit answer. As the most profound philosophy is most cautious against premature philosophizing, so the highest practical sagacity is least inclined to force premature practical results. The most impractical requirement upon science is to limit it to a search for alleged practical results. Wise men do not demand loaves from corn in the blade. Science must be allowed to develop in freedom and bring forth fruit in its season. If what is true makes the best direction for what to do, we may be sure that every truth found will prove itself practical in more and better ways than anyone has thought of hoping for.

Here again, however, Psychologists are not working without aim. The most practical questions, the questions in which men generally are most intensely interested—the questions of health, education, government and religion, which are deepest in the conscious and unconscious life of the world,—these are the questions for which modern Psychology is gathering force. Sagacious men are saying that the next years are to be the Psychological Epoch. There are, at any rate, abundant signs that that intense public interest in science which is always interest in man, even when it is fixed apparently upon some law of Copernicus, Lyell or Darwin, will be challenged next by the results of Psychological research. And it is fair to hope that this will be a schooling no less beneficial than the former ones have been.

The Development of Voluntary Motor Ability.

I.

In studies which involve the development of will, it has usually been thought necessary to begin or at least to conclude with a theory of the source of the force appearing in voluntary motion. This appears not only in works avowedly metaphysical, but scarcely less in works avowedly anti-metaphysical. This is so evident even in strenuously agnostic writings, that metaphysicians may very well appeal thereto as a profound historico-psychological justification of their own occupation.

Concerning the relation between special investigation and metaphysical postulates, explicit or implicit, nothing will here be said. But it is at any rate undeniable that investigators whose *explicit* metaphysical postulates are contradictory, make contributions to a common fund of knowledge. That this is the case even in the study of the development of

will in the individual or in the History of Culture, is a historical fact. How this can be so may appear from the following:

It is evident from the phenomena of growth and it is generally agreed that the activities of a living organism are determined at any point in its history, partly by influences from its environment and partly by the subjective constitution of the organism at that point in its history.

This generalization leaves open the question whether any part of the "subjective contribution" is essentially innate and independent of the rest of nature, or whether all subjective energies have been taken in from the environment and stored up by the individual and its ancestors. Instead of a solution of this metaphysical[1] dilemma, we have in the generalization only the outlines of a scientific task. Whether the metaphysical problem be solved or not, and however it might be solved, the scientific task remains the same. It is the task upon which all students of the growth of living things are in some way engaged.[2]

The study of the development of will in the individual as well as what may be called the development of the "social will" in the History of Culture may be regarded—must be regarded as part of this task. We have, namely, the conditions of a vast experiment. We have on the one hand the world of forces by which the activity and growth of the will are modifiable; an infinite range of things from barometric pressure and cookery, to educational systems and the Spirit of the Times. On the other hand, we have the fact that even the simplest neural reaction is not a simple reflection of the stimulus applied; but that cerebrum, spinal cord, or nerve-muscle machine, each in some degree contributes—from whatever ultimate source—to determine the resulting reaction, and so exhibits something of its own constitution. Every action of animal or man, whether elicited by the "natural" events of life, or by the device of the experimenter, becomes accordingly a source of information about the existing constitution of the subject. It is obvious that we have in this way a general method for studying individual and social development. The same kind of experimentation which shows what

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[1] By metaphysical I mean to describe the knowledge men are held to possess of the absolute nature and source of things. I call this a metaphysical dilemma because its solution seems possible only from a knowledge of the absolute nature and source of the force appearing in the organism.

[2] Among Morphologists, one finds the same dilemma, in the form of a dispute whether or not the embryo has any innate "formative force." Meanwhile men of both views work productively side by side.

the spinal cord can do, shows how much more the cerebrum can do. The same general method of observation which shows the capacity of a child, can follow the enlarging capacity of the child to modify his environment and to shape his own course therein. From the observed reactions or results of reaction of men upon the world, it is possible to write that history of human emancipation, which we call the History of Culture. We have thus a standpoint which leaves open every question as to the absolute nature and source of the forces appearing in action and which, nevertheless, permits the study of the will through every stage of its development, from the events to which the subject contributes.

In gaining this scientific point of view for the study of will, we have at the same time gained a reason for the study of voluntary muscular motion. For to the Psychologist or Sociologist, it can not be an insignificant fact that:

"L'infinie diversité des manifestations extérieures de l'activité cérébrale,..... .....l' hilarité de l'enfant à la vue d'un jouet, le sourire de Garibaldi persécuté pour avoir trop aimé son pays, le tressaillement de la jeune fille à la première pensée d'amour, l'énonciation verbale des lois de Newton .....toutes les manifestations extérieures de l'activité cérébrale se réduisent aux mouvements musculaires." (2)

But the bare fact that all, even the highest, immediate manifestations of the mind are muscular motions does not at once make apparent the deeper justification for the study of those motions in Psychology and the History of Culture. The fact alleged is denied by no one. But many would hold that the motions involved in signing the Emancipation Proclamation, and those which a child might make with the same pen are so different in every respect which concerns Psychology or the History of Culture that their undoubted points of likeness may be neglected as trivial. It is, therefore, necessary to recall the fact that the activities which make up what is called the higher life of humanity are not isolated, but have inescapable connections with the activities and achievements which are usually called lower. It is seen, for example, by discerning men that the development of art, science, philosophy, political institutions—of all that goes under the name of cultivated life, has been made possible, in a large degree, by "material civilization." The more extensively and the more intensively the History of Culture is studied, the more does this historical dependence appear. The progressive attainment of material wealth is necessarily accompanied, in a somewhat corresponding degree, (1) by an increased knowledge of the laws of nature, (2) by an increased amount of force at disposal and an increased skill in its

manipulation, and (3) in consequence, by an increased freedom from the control of the immediate environment.

That the *outer* conditions essential to the development of higher culture are furnished in this way is evident from all historical study, even if it were not generally realized in the personal experience of men devoted to any form of cultivated life. Any work requiring leisure must have the leisure provided by some form of stored work. It is practically impossible for art or science to flourish except by help of the stored work, which material civilization has provided. "Before we can live well, we must manage to live."

It is less evident but more, rather than less important that the *inner* conditions of higher culture are prepared by the struggle for material wealth. The earth does not give up its wealth without teaching something of its laws. This knowledge may not be so extensive, so precise or so well organized as that which we at present call scientific. But it has one high mark of truth. It works. Which means that within important limits it is true. Moreover, if not so extensive as science, it is generally more intensive. It is less knowledge than practical wisdom. Such as it is, it is the subsoil out of which all higher forms of culture grow.

To a sufficiently superficial view, the most essential requisite of this industrial civilization appears to be machinery, for it is only by machinery that men are able to control indefinitely great force with indefinitely great precision, and so to gain their indefinitely great ascendency in the world. The smallest penetration shows, however, that the one essential machine by which all other machines have been made, and for which all other machines are supplements, is the nerve-muscle apparatus. The bare-handed man has at disposal comparatively little force. He can manipulate this force with comparatively little precision, either in space, in time or in intensity. He has accordingly comparatively little freedom; comparatively small ability to modify his environment and to help determine his own course therein. Much or little, however, this power and skill and consequent freedom are the fundamental capital of life. All greater powers through machines, all finer skills, through instruments of precision, all larger freedoms up to the highest which men enjoy, have their essential pre-condition and their prototype in the howsoever modest attainments made in the individual body. When, besides, it is remembered that the life-long and world-long expression of thoughts and feelings solely through muscular movements makes muscle habits infinitely the most subtle and complete record of the conscious life, and when it is remembered, further, that these muscle habits constantly react with deter-

mining power upon the whole activity and growth of the mind, it is not enough to say that the subject is entitled to study from the standpoint of Psychology and the History of Culture. It must rather be said that these sciences will be obliged to study the development of motor ability. We have a right to expect from such study a typical chapter in the whole progress of man, a "grammar of will." And we have therefore a right to hope from such study new and fundamental approaches to the understanding of the History of Culture.

Inasmuch as voluntary motions are data at once for Physiology, Psychology and the History of Culture, one might expect to find that they had already received attentive study from all, or at least from some, of these standpoints. In fact, however, the point of common interest has been a point of common neglect. Students of the History of Culture have found overwhelmingly abundant material for research and speculation in the results,—the records of human reaction upon the world—tools, buildings, works of art, languages, books, rites, governments, etc. They have accordingly been able to overlook the muscular motions through which alone these "works" have arisen. The manifest importance of the sociological material, moreover, has made muscular motions, as such, seem comparatively elementary and trivial. It may accordingly be understood why it is only in rare instances — as in one department (phonetics) of the old and highly developed science of philology — that we have any considerable study of muscular motion.

If the study of voluntary motion has been postponed in Sociology because of its simplicity, it has been postponed in Physiology for just the opposite reason. "Man kann behaupten," says Fick, "dass die ganze thierische Organization jene ('willkürliche') Bewegungen zum Zwecke hat" (35). But of the "riesiges Material von Versuchen über Muskelzusammenziehung," of which he speaks (p. 2), nearly all is engaged with the more elementary phenomena of nerve-muscle action rather than with what von Kries calls "resultirende Bewegungen." Von Kries says (3):

"Einer Untersuchung der willkürlichen Muskelthätigkeit bieten sich wesentlich zwei vershiedene Aufgaben, welche, wiewohl in naher Beziehung zu einander, doch sorgfaltig unterschieden werden müssen. Wir können zunachst die Bewegungen beobachten welche sich an den festen (knöchernen) Theilen des Körpers durch willkürliche Muskelthätigkeit hervorbringen lassen; wir wollen sie kurz die resultirenden Bewegungen nennen. Da es shon bekannt ist, dass diese

Bewegungen in der Regel durch ein verwickeltes Zusammenwirken vieler Muskeln bewirkt werden, so erhebt sich als weitere Frage die nach der Thätigkeit der einzelnen Muskeln. In dieser letzen Hinsicht steht seit geraumer Zeit die Frage nach gewissen zeitlichen Verhältnissen insbesondere nach der Stetigkeit oder Discontinuität, eventuell nach dem Rhythmus der Innervation, im Mitteltumkt bes Interesses, ohne jedoch bis jetzt abschliessend beantwortet zu sein. Aber auch in der ersteren bieten sich gewisse einigermaassen ähnliche Fragen welche mir ein selbständiges Interesse zu verdienen scheinen."

The efficiency of a machine depends, so far as we know, upon the maximum force, rate, amplitude, and variety of direction of its movements; and upon the exactness with which, below these maxima, the force, rate, amplitude and direction of its movements can be controlled. The motor efficiency of a man depends upon his ability in all these respects. All of them are determinable within varying limits of precision. All of them have been made objects of research more or less limited. But an adequate determination in respect to any of them, either for the average adult, for children at successive stages of development, or for the sick or aged in in successive stages of decline, is wanting.

The following research deals with the development of voluntary motor ability of children with respect to

1. The maximum rate of rhythmically repeated movement.
2. The precision of voluntary movement, particularly as regards direction and force.
3. With a note on the bilateral development of strength and endurance.

The Maximum Rate of Voluntary Movement.

LITERATURE.

1. *The maximum rate of inervation.*

The maximum rate of inervation has been reported differently as follows:

Helmholtz([4]), 1866, 18–20 per second.
Hall and Kronecker([5]), 1879, about 20 " "
Horsley and Schaefer([6]), 1886, 10 " "
Schaefer, Carney and Turnstall([7]), 1886, 8–12, Av. 10 per sec.
Von Kries([3]), 1886, 11–12.4 per second.
Griffiths([8]), 1888, 8–21 per second (see below).
Haycroft([9]), 1890, about 19.5 " "

H. thinks that the muscle vibrations "cause and compound themselves" with rhythms in the instruments used, and so endeavors to explain former contradictory results.

2. *The Maximum Rate of "Resulting Movements."*

Von Kries([7]).

Shortest movement of middle finger (Av. 11 trials), .077.
Hand, (Av. 10 trials), .074.
Tongue, .066.
Foot (plantar flexion), .125"–111".
Jaw, 125"–111".

Maximum rate of rhythmically repeated movement after practice, 10–11 per second.

Vocal organs about same as hand.

Respiration in dogs has been observed at 7 per second.

Cattel and Fullerton([10-1892]), for a movement of 50 cm. time varies from 87" to 118" in four individuals.

Dresslar([11-1892]) (when 300 taps were made), 6.5–10.5 per second.

For a short time, 11 per second.

Dresslar gives the records of 27 adults. The average of these records is about 6 per second; 300 taps were made in each case.

3. *Influences Affecting the Rate of Movement.*

Horsley and Schaefer([5]). The rate of the muscle rhythm is the same when cortex or spinal centers are electrically stimulated and when the muscle is voluntarily stimulated.

Griffiths([8]). The rate of voluntary muscle rhythm varies in different individuals, different muscles, and with fatigue.

Dresslar([11]). Muscular exercise lowers the rate; mental excitement increases the rate. There is a daily rhythm with the rise and fall of mental and nervous tension.

Cattell and Fullerton([10]). Women have decidedly slower rate than men. The rate is very constant.

Von Kries([7]). The rate varies slightly in different muscles; increases with practice; but is not affected by variation of the amplitude of motion within wide limits, a certain medium amplitude requiring less time than longer or shorter distances.

(See v. Kries' tables (op. cit. p. 4), where it is shown that excursions of 10 mm. and 16 mm. are made by the middle finger in less time than are excursions of 4 mm.; that excursions of 19 mm. and 25 mm. are made in less time by the hand than are excursions of 9 mm.; and that a decided increase in time over that required for excursions of 4 mm. or 5 mm. does not appear in excursions of less than 30 mm.)

Camerer([12]), 1866.

The will brings about an intended rate of movement only gradually. Constant rate of motion is unnatural and forced. The natural rate of motion is one of constant acceleration.

Conclusions from the Literature.

The maximum rate of voluntary muscle rhythm is not satisfactorily determined.

The maximum rate of voluntary rhythmically repeated "resulting movements" in adults has been found in some cases 11–12 per second. The average maximum rate of adults is not determined.

The maximum rate varies with individuals, with muscles used, with fatigue, and with mental excitement; but not within wide limits, with the amplitude of the movement.

FIG. 1.—Scheme of Mechanical Counter.

Apparatus (see Fig. 1). A spring clock movement (cost $1.00) was taken out of its case. The balance wheel with its spring was removed. The axle (b) which supports the escapement lever (c) had rigidly attached to it, and extending about four mm. horizontally from it, and at right angles to it, a strip of brass (a). The outer end of this strip was fastened to a light wooden lever (e). The attachment between the two was made by means of another strip of brass (d) about 5 mm. long, fastened to each by fine flexible wire loosely enough to allow necessary play. The fulcrum (f) of the lever, distant about 34 mm. from the clock, was supplied by a Morse key, the arm of the key forming a rigid continuation of the lever attached to the clock. The key and the clock were each firmly fastened to the same wooden base at a distance determined by the length of the lever. The end of the lever attached to the clock, was made to occupy a position directly above its point of attachment, at a distance determined by the length of the connecting strip. This distance demands rather delicate adjustment, in order that the upward and downward movements of the lever may cause a properly balanced upward and downward movement of the escapement lever. When proper adjustment is made, taps upon the button of the Morse key permit the 'scape wheel to revolve, one cog and only one, for each tap. To prevent possible errors in the train of wheels connecting the escapement and the second hand, a hand was attached to the axle of a second's wheel, which engages directly with the 'scape wheel, the clock face being secured in a corresponding position. It requires 120 taps to permit the hand to make one revolution.[1]

[1]There are 48 cogs on the second's wheel engaging with 6 cogs on the axle of the 'scape wheel. There must therefore be eight revolutions of the 'scape wheel to permit one revolution of the second's wheel. And since there are 15 cogs on the 'scape wheel, there must be 8 x 15 escapements in order that the second's wheel may revolve once.

The distance through which the button of the Morse key must be moved depends of course upon the length of the lever employed. The maximum vertical movement of either pallet of the escapement lever, in the clock used, is 1.5 mm. The depth of the cogs of the 'scape wheel is 1.1 mm. The distance from weight to fulcrum, and from fulcrum to power, being 34 mm. and 9 mm. respectively, the minimum and maximum movements at P must be respectively .29 mm. and .39 mm. The amount of force required is insignificant.

The accuracy of the apparatus was tested by placing the Morse key in a circuit with a Duprez signal, in position to write upon the kymograph. It was found in many trials that the number of taps recorded upon the clock face corresponded with the number of taps recorded upon the drum. An (undescribed) apparatus devised by Dr. E. C. Sanford, differing from that described in that the lever is attached to the armature of a magnet and moved to and fro by the counter action of an electric current and spring, was tested by the Kronecker interrupter and proved correct to 20 counts per second. As the two devices are essentially the same, except in the mode of moving the lever, this test indicates the probable capacity of the instrument used by me. A few of the measurements hereafter described were made with Dr. Sanford's apparatus.[1]

The Course of the Experiments.

All the joints of the upper extremities were tested. The conditions of the tests were as follows:

Shoulder.

The forearm being held at right angles to the upper arm, and the back of the elbow being held in position above the button of the Morse key, the upper arm was caused to move up and down as rapidly as possible in a plane nearly parallel with the vertical plane of the body, i. e., in about that plane in which the arm tends to swing when one walks.

Elbow.

The elbow rested upon the table. The forearm was held at right angles to the upper arm. The key was struck with

[1]The use of reaction time as a general clinical test has, it is well known, been made difficult by the cost of apparatus and by the delicate manipulation required to secure reliable results. If it should prove true, as now seems probable, that the rate of voluntary movement is a valuable supplement to the reaction time test—if not also in many cases a good substitute therefor—these difficulties will not be met. The apparatus which I have described, for example, can be made at a cost of two or three dollars and will give reliable results without more time or care in manipulation than many clinical tests in general use require.

the ventral side of the forearm just back of the wrist. In a few instances the arm was extended and while the elbow rested upon the table, as before, the taps were made with the little-finger side of the wrist. No difference appeared in the rate of the two motions. It is to be noted that the school experiments were made with the ordinary (Spring) clothing about the arm. In my own case, the removal of my coat made no discernible difference in the rate of elbow or shoulder, owing, I think, to the great disproportion between the force of the limb in movement and the resistance which ordinary clothing presents.

Wrist.

The elbow rested upon the table. An iron clamp, whose jaws were covered with firm cushions of cloth over cotton, was placed rather loosely about the forearm just back of the wrist joint, and was held in position by an ordinary stative. The key was struck with the palm of the hand.

Metacarpo-Phalangeal Joint of Forefinger.

The palm of the hand was held with moderate firmness at an angle of about 135° with the forearm. The finger in position was then nearly or quite parallel with the forearm. In this way possible sympathetic movements of the wrist were prevented from affecting the record. If properly placed, the clamp does not interfere in the least with free movement of the finger. Other phalangeal joints were tested only in the case of adults. A narrow clamp was then used and, as in the case of the hand, interference with other joints was prevented in part by the position in which the member was held.

In all the experiments reported in this paper, the maximum number of taps in five seconds was determined, and all results are given in terms of x taps in five seconds. A stop watch, or rather a timer, measuring fifths of a second, was used to measure the time. In 60 seconds this timer shows no discernible variation from a standard second's pendulum. For measuring periods of five seconds, therefore, the readings do not differ from those which could be obtained from a theoretically perfect instrument.

In the case of adults, the subject, being in proper position with reference to the tapping apparatus, and with the timer before him, began to tap as the watch hand passed a five-seconds mark upon the dial, and ceased tapping as the hand passed the next five-seconds mark. It is obvious that each time, the personal errors at the beginning and at the end of the interval tend to balance each other, and that, in the long run, the plus and minus errors in this balance tend to balance each other. In the school experiments the starting and stopping followed a word of command. The timer was

started as nearly as possible at the same instant in which movement was observed to begin. When five seconds had elapsed the command to stop was given and any subsequent tap was not counted. The reaction time of the pupil was thus not included, and the observer's errors tend to balance as above. The error from this source can scarcely exceed one tap in a single test.

To prevent incipient fatigue, slight pauses were made between each five-second period of work, with longer pauses every second or third time.

All the rate tests were taken by myself, except possibly a dozen taken by my wife, who assisted me throughout every part of the present research, and who was thoroughly familiar with every detail of the work.

Preliminary Experiments.

Besides furnishing a test of the apparatus and method used, the preliminary experiments upon adults show some important characteristics of the rate of voluntary motion.

1. The rate of voluntary motion in a given joint of a given individual is very constant.

The following tables taken at random from many, show the degree of variation in individual successive measurements.

(Explanation of tables: I., outer joint of the forefinger; II., middle joint of forefinger; III., metacarpo-phalangeal joint of forefinger; IV., wrist; V., elbow; VI., shoulder; VII., free tap. Each number in the tables shows the number of taps made in a period of five seconds.)

Table I. Subject, L. B. February 18, 1892.

	I	II	III	IV	V	VI	VII
Right.	27	28	35	37	34	33	34
	26	27	35	34	33	29	38
	28	30	35	34	33	29	38
	28	27	33	38	34	29	35
	32	30	36	35	33	34	34
Left.	27	26	31	34	31	25	38
	31	28	30	36	32	25	31
	27	26	29	35	34	25	35
	26	28	30	33	36	26	32
	28	28	30	34	34	25	33

SUBJECT, E. C. S. FEBRUARY 27.

	I	II	III	IV	V	VI	VII
Right.	19	30	30	39	35	32	38
	17	29	30	37	32	24	43
	26	22	28	37	27	29	41
	23	25	30	36	37	25	39
	22	29	30	34	30	27	37
Left.	16	18	26	27	24	22	26
	17	16	25	25	29	18	31
	17	17	23	25	21	21	27
	18	15	26	27	22	17	30
	16	18	23	24	23	18	27

SUBJECT, W. B. FEBRUARY 29.

	I	II	III	IV	V	VI	VII
Right.	23	23	25	29	39	28	36
	23	22	24	27	41	30	38
	23	23	23	23	40	20	36
	22	23	27	30	37	28	40
	22	21	24	27	39	28	39
Left.	17	15	20	20	24	17	27
	15	16	20	21	23	22	26
	15	18	20	21	27	21	24
	11	18	22	24	25	17	24
	11	17	23	23	25	23	26

Mean Variation of Individual Results from the Mean.

In the case of 239 mean rates, each obtained from five single tests on W. B., the mean value of the mean individual variations $\left(\frac{\Sigma v.}{n}\right)$ is .85 taps in five seconds; and two thirds of the $\left(\frac{\Sigma v.}{n}\right)$ values are less than 1.1 taps in five seconds. In 82 such mean rates obtained from L. B., the mean value of $\left(\frac{\Sigma v.}{n}\right)$ is 1.09 taps in five seconds and two thirds of these $\left(\frac{\Sigma v.}{n}\right)$ values are less than 1.4 taps in five seconds. Of 355 mean rates obtained from three subjects, 96% show $\left(\frac{\Sigma v.}{n}\right)$ values less than two taps in five seconds. So far as these experiments have

weight accordingly, the probability is .96 that two records of the maximum rate of voluntary movement, taken as nearly as possible under the same outer and inner conditions, will differ less than two taps in five seconds. It is altogether likely that there are individuals in whom the variability would be somewhat greater.[1]

2. The rate of voluntary movement undergoes slight and gradual but measurable changes due to changes in the subject.

Effects of Local Cold. The application of snow to the left forefinger resulted in reductions of the rates of the joints of that finger, amounting to 1.6, 1.6 and 1.4 taps in 5″; but caused no corresponding change in the rates of the other joints.

Effect of Local Fatigue. Fatigue was induced by rapid and continuous voluntary movement of the joint, in the same manner as that required in tapping with that joint. Sometimes fatigue was hastened by weighting the joint. Tests were taken from time to time after ten minutes' work. The final tests were taken after one to three hours' work. In one case (Table III.), fatigue was induced in the left hand by gripping upon the Galton dynamometer. In all cases the process becomes excessively painful. Following tables give the results gained. Explanation of tables: The joints are indicated by the Roman numerals from I. to VI., beginning with the outer finger joint. VII. indicates the free tap. R=Right side. L=Left side. The exponent a means that the series following was taken before fatigue. The exponent w indicates that every single record represented in the series of averages following was taken while one of the joints upon that side was in state of extreme fatigue. The exponent b indicates that every single record represented in the series of averages following was taken while one of the joints upon the opposite side was in a state of extreme fatigue. The wearied joint and the corresponding joint on the other side are indicated by underscoring their records. The records of other joints upon both sides are given to show the general motor ability before and during the local fatigue. For convenience of reference, each set of results has been numbered. In (33) a was taken at the point

[1] The constancy of the maximum rate of motion is indicated by the small limits within which the racing records of a given individual vary. Notwithstanding the very large number of motions made by a horse in running one mile, a dozen successive race-records are not expected to have a gross variation of more than two or three seconds, if the horse, the track, the weather, etc., are each time in about the same condition. The same holds true of bicycle riders, oarsmen etc.

of extreme fatigue, b after an interval of recovery. In (36 and 38) a, b, c, and d were taken in order at various periods from thirty minutes to two and one-half hours. In (42) a, b and c were taken at periods of fifteen, forty-five and one hundred and fifty minutes, work being continued all the time.

TABLE II.

RATE OF TAPPING. I. P. 1–6. RECORD W. B.

	I	II	III	IV	V	VI	VII	No.
Feb. 2, R^a	22.6	22.4	24.6	29.6	39.2	28.6	38.	1
	.4	.7	1.1	1.8	1.1	.6	1.4	
" 2, R^b	20.2	19.8	23.2	28.8	35.4	26.2	38.8	2
	.3	.7	.2	1.	1.6	1.7	.6	
" 2, L^w	13.4	16.6	21.	21.4	24.8	20.6	25.4	3
	1.9	.9	.6	.9	.1	1.6	1.9	
" 5, R^a	19.2	22.2	29.6	33.	37.2	31.	40.	4
	.7	.6	.9	1.1	.3	2.8	1.6	
" 5, R^b	18.4	19.6	22.8	28.4	34 8	30.2	38.2	5
	.6	.5	1.0	1.2	.9	1.8	1.	
" 5, L^w	18.4	18.2	18.8	21.4	27.4	23.8	25.8	6
	.6	.7	1.	1.1	.9	1.4	1.	
" 8, R^a	21.	22.6	26.6	31.4	35.8	28.8	38.	7
	.4	1.1	.5	1.9	1.4	1.4	.8	
" 8, R^b	21.	20.6	26.	28.4	34.	30.2	36.2	8
	.8	1.7	.4	2.	.2	1.	.6	
" 8, L^w	19.4	18.6	22.	23.8	27.2	27.4	28.2	9
A.M.	1.	.6	.8	.6	1.5	1.1	1.4	
" 9, R^a	19.8	19.2	25.6	31.8	38.2	28.4	38.6	10
	.3	1.7	1.2	1.7	2.	.9	1.1	
" 9, R^b	20.2	19.6	27.2	30.8	30.2	29.4	33.	11
	.5	.9	.7	.7	.2	1.1	2.4	
" 9, L^w	18.6	19.	22.	25.8	23.2	25.2	25.6	12
P.M.	.9	.4	1.2	1.1	1.	1.	.9	
" 9, R^a	20.8	21.8	26.8	29.4	34.8	29.2	36.8	13
	1.	.6	1.4	1.0	.6	1.	1.1	
" 9, R^b	21.3	21.	26.6	29.6	31.8	25.8	36.2	14
	.2	1.2	.7	.5	1.4	1.4	.6	
" 9, L^w	18.	20.	22.4	25.	25.2	20.6	25.8	15
A.M.	.4	.8	.8	1.2	.3	1.1	1.0	
" 11, L^a				24.8	26.6	21.4	27.4	16
				.8	.5	1.4	.9	
" 11, L^b				23.0	24.1	21.6	25.8	17
				.8	.2	1.1	.6	
" 11, R^a				33.	35.6	27.6	37.4	18
				.8	1.9	1.1	1.7	
" 11, R^w				30.6	28.8	26.4	30.2	19
P.M.				.6	1.7	.5	2.1	
" 11, L^a			23.6	25.2				20
			.5	.3				
" 11, L^b			21.3	23.9				21
			.6	.5				
" 11, R^w			21.0	31.				22
			.5	.1				

TABLE II.—*Continued.*

RATE OF TAPPING. RECORD W. B.

	F. Finger.			M. Finger.			R. Finger.			L. Finger.			Thumb.			
Feb. 10	I	II	III	I	II	III	I	II	III	I	II	III	I	II	III	
R^a	22.6	22.6	25.8	21.6	23.2	26.4	20.2	20.6	24.8	20.	20.4	24.8	20.6	21.	23.8	23
	1.1	1.1	.6	1.3	.3	.9	1.1	1.3	.3	1.2	1.1	.6	1.6	.8	.6	
L^a	18.8	20.4	22.4	13.2	17.8	22.	12.4	17.2	19.6	13.4	16.2	18.	17.8	16.4	18.6	24
	.6	.3	.5	.6	.6	.4	.7	.3	.9	.9	.1	.8	.6	.5	1.3	
L^b	17.4	18.4	19.2			21.6			20.2			18.6			20.	25
	.5	.1	1.			.6			.3			.9			1.2	
R^w	22.2	21.6	24.6			26.			24.2			22.6				26
	.7	1.5	.8			.8			.7			.7				

RATE OF TAPPING. RECORD W. B.

Feb. 17	I	II	III	IV	V	VI	VII		
R^a	21.2	21.2	25.8	32.6	37.8	27.4	38.		27
	.7	.6	.3	1.4	.7	1.4	1.2		
L^a	18.	18.8	23.4	24.2	27.	22.	26.6		28
	.5	.6	.5	1.9	.4	.4	.7		
R^w			25.8	32.6	32.2	26.4	36.8		29
			1.	3.6	1.4	1.3	2.5		
L^b			22.4	23.	25.	23.	26.		30
			1.1	.8	.8	.4	.4		

RATE OF TAPPING. L. B. RECORD.

Feb. 17	I	II	III	IV	V	VI		
Left A.	23.2	25.6	30.8	31.6	31.6	25.2		31
	1.4	1.7	1.9	3.	.8	.7		
R^a	24.6	26.1	34.3	36.	35.3	31.6		32
	.6	1.6	.4	.2	1.0	.7	A's taken first.	
	B	B		B	A	*B*	B's after an interval of recovery.	
R^b	27.2	28.	29.4 31.6	34.8	34.8	30.		33
	1.4	.8	1.2 1.3	1.	.7	1.2		
	B	*B*	A *B*	*B*	*B*	*B*		
L^w	26.	27.1	25.7 29.6	33.4	32.2	26.2		34
	.4	1.4	.3 1.3	1.4	2.1	1.6		

TABLE II.—*Continued.*

RATE OF TAPPING. RECORD F. B. D.

Feb. 18 7 P. M.	I	II	III	IV	V	VI	VII	
R[a]	21.6	30.	33.1	55.2	44.	26.	61.8	35
	.4	1.3	.5	4.1	3.8	1.2	.6	
R[b]	20.	33.3	(a) 54.	50.	49.	26.	58.5	36
	0.	.3	5.3	0.	1.	0.	1.	
			(b) 52.3					
			3.9					
			(c) 50.6					
			3.					
			(d) 50.6					
			.3					
L[a]	19.	24.2	37.6	40.6	38.	24.	41.	37
	0.	.7	1.	1.3	2.5	.6	2.	
L[b]	20.6	25.	(a) 29.3	41.	34.	25.	42.	38
	.8	1.	2.4	1.	.6	0.	2.	
			(b) 30.2					
			5.2					
			(c) 30.7					
			.7					
			(d) 34.					
			1.7					

RATE OF TAPPING. RECORD E. C. S.

Feb. 27	I	II	III	IV	V	VI	VII	
R[a]	21.	27.	30.7	36.4	32.8	27.4	39.6	39
	2.6	2.8	2.2	1.2	1.7	2.5	1.9	
L[a]	16.8	16.8	24.8	25.6	23.8	19.2	28.2	40
	.6	1.	1.3	1.1	2.1	1.8	1.8	
R[b]	23.4	25.4	27.3	37.4	45.*	26.	40.8	41
	2.1	1.8	1.2	1.3	6.4	2.2	1.4	
L[b]	16.2	18.6	(15 m.) 20.8	26.8	24.2	21.6	29.4	42
	.3	.6	.6	1.	1.8	2.	1.9	
			(45 m.) 19.8					
			.6					
			(2½hrs.) 18.4					
			1.2					

*The individual rates were 54, 39, 39, 50, 38. The 54,50 rates were visibly the shiver of "voluntary tetamus."

TABLE III.

RATE OF TAPPING. WEARYING BY GRIP OF LEFT. W. B.

	I	II	III	IV	V	VI	
R^a	21.	23.	25.	28.	34.	32.	43
R^b	21.	24.	25.	28.	38.	30.	44

1. Fatigue begins to show itself by a perceptible lowering of rate after ten or fifteen seconds' work.

This fact appeared so clearly that it proved necessary to allow a brief interval of rest between each five-second period of work, and longer intervals every second or third time.

(NOTE—See in Dresslar's Influences affecting Rate, the evidence of fatigue in the course of 300 taps.)

2. After ten to fifteen minutes' work the reduction in rate is considerable. Thereafter the reduction goes on more slowly. By three hours' work E. C. S. reduced his left wrist to momentary helplessness.

3. Partial recovery takes place very quickly.

This fact came out with such certainty that it proved necessary to keep the wearied joint hard at work throughout the series except during the 5″ intervals when a test was in progress.

4. Complete recovery from excessive fatigue takes place slowly.

Compare Nos. 10-12 with 13-15, Table II., for evidence of the persistence of fatigue during 2-4 hours' rest.

5. Working a joint induces a local fatigue, which does not perceptibly affect the rate of remote joints. In some cases the fatigued joint's rate is reduced, while nearly all the other joints show an increase of rate.

6. Taking the average yearly increase of rate as a standard, the change of rate induced by fatigue is very great.

It will be shown later on, that between the ages 6 and 16 the average yearly increase of rate ranges from .74 to 1.44 taps in five seconds, and that a yearly gain of two taps or over in five seconds has a probability of .17 or .33 less than an even chance. The decrease of rate by local fatigue induced by 1 to 2½ hours' hard work is shown to be as follows:

W. B., 5, 4.6, 6.8, 1.2, 5.6; L. B., 5.1; F. B. D., 8.3; E. C. S., 5.

As noted above, by 3 hours' work, E. C. S. reduced the rate of the left wrist practically to zero.

In all but one instance these amounts are equivalent to the growth of three or four years.

7. I am not prepared to say positively whether local fatigue in a joint affects the rate in the corresponding joint on the other side. The facts are as follows: In my own case (as shown in the W. B. records) in every trial (left shoulder, elbow, wrist, hand-finger joint and middle finger joint; and with the right elbow, wrist and hand-finger joint), extreme local fatigue in a joint was followed by a diminution in the rate of the corresponding joint on the other side, while other joints, upon both sides, showed no corresponding change in general motor ability.

As results in which the experimenter is the subject, are always justly subject to doubt because of the possible unconscious influence of expectation, I observe,

(a) In Table III., where it is shown that wearying the left hand by gripping was followed by no discernible change in rate of any right side joint, the contrary result was fully expected.

(b) In my case, the most rapid joint is the elbow. The rate of the free tap is, therefore, mostly determined by the rate of the elbow. It will be seen in each case that the rate of elbow and free tap closely agree. If, now, those cases in which either elbow was wearied be examined, it will be seen not only that the rate of the other elbow is lowered, but also that the rate of the free tap on the other side was lowered. This result was not anticipated, and only came to notice when calculations and comparisons were made subsequent to the conclusion of the experiments.

(c) On February 9th, A. M. (Table II., 10-12), the left elbow was wearied, the experiment concluding about noon. At about 2 P. M. of the same day work was resumed, the left shoulder being wearied. The record for February 9th, P. M. (Table II., 13-15), shows that neither the left elbow fatigued in the morning nor the right elbow have recovered their normal rate, although the rate of other joints proves that there was no general decline of motor ability. This result was not anticipated and was not discovered until later.

The results from L. B. confirm the results from W. B.

The results from F. B. D. show an extraordinary increase in the rate of the joint corresponding to the wearied joint, in the presence of no considerable change in the rate of other joints on either side. This wholly unexpected result was thoroughly verified.

The results given from E. C. S. appear to confirm the result from W. B. In a second very thorough trial, the left wrist being fatigued to the point of temporary helplessness,

no bilateral effect appeared. I have had the misfortune to lose this valuable negative record. I have been strongly inclined on this account to withhold all results on this point from publication. I have concluded to publish the results, specially emphasizing the entirely negative result from E. C. S. and withholding, until further investigation, any positive conclusions as to the question involved.

The Effect of Amplitude of Movement on Rate. I have already referred to the conclusion of von Kries (p. 138), that "the extent of the movement has small influence upon its duration," motions of a certain medium extent appearing to be carried out more quickly than longer or shorter ones. With this result my observations are in entire agreement. The following table gives the number of double excursions of the several amplitudes indicated in five seconds:

TABLE IV.

Extent of Excursion in mm.	1	5	10	15	20	25	30	40
Number of double excursions in 5 sec.	27	26	27	30	30	30	25	20
	28	29	28	30	29	30	26	23
	24	28	28	29	30	30	24	24
	24	29	30	28	30	29		26
	27	32	29	31	31	28		
	27	29						
	25							
Average.	26.	28.8	28.4	29.6	30	29.4	25	23.2

Maximum Rate of Tapping by Adults. A sufficient number of adults have not been tested to permit the establishment of a normal mean rate or to show the individual variation. For the results with unwearied joints see Table II. Nos. 1, 4, 7, 10, 13, 16, 18, 20, 23, 24, 27, 28, 31, 32, 35, 37, 39, 40 and 43.

SCHOOL EXPERIMENTS ON RATE.

(NOTE—I am indebted to Superintendent Marble and the school authorities of Worcester, and to Dr. Franz Boas, for the opportunity of making these tests.)

Seven hundred and eighty-nine school children of the City of Worcester, Mass., ranging in age from 5 to 16 years, were tested with the apparatus and by the method described. The shoulder, elbow, wrist and metacarpo-phalangeal joint of the forefinger on each side were so tested.

Classification of Results.

Forty-six individuals (twenty-six boys and twenty girls) were more or less left handed. Their records are taken account of separately (175). Twenty individuals (fourteen boys and six girls) of five years of age were tested. The boys' record is printed in Table VI. The principal calculations and results of this paper refer to the 723 right-handed individuals ranging in age from 6 to 16. The 5,784 single results obtained from these individuals were classified according to the age and sex of the individual, and according to the side and joint used.

Method of Treating Results.

The several results belonging to each class,—for example, to the right finger of the boys of six,—were tabulated so as to show the different rates found in that class (Column I.), (Table V. page 152) and the number of times each of these rates occurs (Column II.). Each rate found was multiplied by the number of times it occurs; and the sum of these products (Column III.) was divided by the sum of individual cases. The differences between this result, the arithmetical mean, and each rate found were taken (Column IV.), and these residuals were squared (Column V.). Each squared residual was multiplied by the number of times the corresponding rate occurs. The sum of these weighted squared residuals (Column VI.) was divided by the number of individual cases. The square root of this quotient is the mean individual variation, showing the limits of distance from the mean, within which 68.3% of the individual results fall. This individual mean variation divided by the square root of the number of cases, gives the mean variation of the mean. The probability is .683 that the true mean lies within the limits thus determined.[1]

The tabulation and calculation for boys' right finger, 6, are given as an example. A designates the arithmetical mean; μ the mean individual variation and μ_o the mean variation of the mean. The values of A, μ, and μ_o for each class, are given in Table VI. page 152, andare represented graphically in chart I. In the latter the values of A are connected by

[1] The so-called probable errors, i. e., the errors whose probability is .50, may be found by multiplying the values of μ and μ_o by .6745.

solid lines, and the values of μ and μ_o by dotted lines on each side of the solid line.[1]

TABLE V.

RATE BOYS 6. RIGHT FINGER. n=26.

Rates found = R.	No. of cases at each rate = n.	R × n.	Residuals. = v.	v^2	$nv.^2$
I	II	III	IV	V	VI
15	1	15	4.5	20.25	20.25
16	1	16	3.5	12.25	12.25
17	1	17	2.5	6.25	6.25
18	4	72	1.5	2.25	9.00
19	3	57	.5	.25	.75
20	10	200	.5	.25	2.50
21	3	63	1.5	2.25	6.75
22	2	44	2.5	6.25	12.50
23	1	23	3.5	12.25	12.25
	26	507			82.50 $=\Sigma v.^2$

$507 \div 26 = 19.5 = A.$

$$\sqrt{\frac{82.50}{26}} = 1.78+ = \mu_R$$

$$\frac{1.78}{\sqrt{26}} = .34+ = \mu_{OR}.$$

[1] It must be kept in mind that the mean rates do not represent quantities which exist in nature. Each mean is a function of the individual rates found in a given class, and only in connection with the mean individual variation and the mean variation of the mean itself, does it represent the probable rate and distribution of rates for that class.

TABLE VI.

RATE: AGE 5.

		BOYS, n=14.			GIRLS, n=6.
		A	μ	μ_o	Omitted, because so few.
R	F	19.6	2.82	.8	
	W	20.1	3.66	1.0	
	E	22.7	2.90	.8	
	S	18.4	2.72	.7	
L	F	17.3	3.34	.9	
	W	17.	3.39	.9	
	E	18.2	3.23	.9	
	S	17.	2.38	.6	

RATE: AGE 6.

		BOYS, n=26.			GIRLS, n=28.		
		A	μ	μ_o	A	μ	μ_o
R	F	19.5	1.78	.3	19.8	2.56	.5
	W	23.	2.72	.5	21.6	2.57	.4
	E	23.5	1.45	.3	22.7	2.33	.4
	S	19.8	2.81	.6	19.9	2.71	.5
L	F	18.	1.56	.3	18.	2.76	.5
	W	19.7	2.32	.5	18.9	2.33	.4
	E	20.4	2.47	.5	19.7	2.06	.4
	S	18.2	2.34	.5	17.9	2.43	.5

RATE: AGE 7.

		BOYS, n=35.			GIRLS, n=32.		
		A	μ	μ_o	A	μ	μ_o
R	F	21.	2.64	.4	20.7	2.46	.4
	W	23.7	2.91	.5	23.1	2.7	.5
	E	24.2	3.71	.6	23.2	1.95	.3
	S	20.5	2.48	.4	20.2	2.66	.5
L	F	19.1	2.4	.4	19.1	3.23	.6
	W	20.2	2.6	.4	20.	2.46	.4
	E	20.9	3.07	.5	21.5	2.96	.5
	S	18.8	2.5	.4	18.8	2.67	.5

TABLE VI.—*Continued.*

RATE: AGE 8.

		Boys, n=33.			Girls, n=32.		
		A	μ	μ_o	A	μ	μ_o
R	F	23.1	2.56	.4	22.2	2.74	.5
	W	26.3	2.83	.5	24.3	2.73	.5
	E	26.1	2.57	.4	24.4	2.68	.5
	S	22.3	2.26	.4	21.9	3.08	.5
L	F	20.5	1.91	.3	19.7	2.71	.5
	W	22.2	2.88	.5	21.	2.69	.5
	E	22.3	2.61	.4	21.6	2.27	.4
	S	20.2	2.69	.5	20.2	2.64	.5

RATE: AGE 9.

		Boys, n=43.			Girls, n=36.		
		A	μ	μ_o	A	μ	μ_o
R	F	24.4	4.15	.6	24.	2.69	.4
	W	27.8	3.5	.5	25.5	3.26	.5
	E	28.2	3.52	.5	25.4	3.42	.6
	S	24.1	3.65	.6	22.7	3.26	.5
L	F	21.5	3.6	.5	20.6	2.92	.5
	W	23.6	3.84	.6	22.4	2.99	.5
	E	23.8	3.67	.6	22.6	2.91	.5
	S	20.9	3.57	.5	20.8	3.79	.6

RATE: AGE 10.

		Boys, n=37.			Girls, n=35.		
		A	μ	μ_o	A	μ	μ_o
R	F	25.2	2.29	.4	25.8	3.36	.6
	W	28.5	3.34	.5	28.5	3.66	.6
	E	28.1	3.60	.6	27.5	2.96	.5
	S	22.6	2.75	.4	22.6	2.76	.5
L	F	22.4	3.15	.5	22.3	2.64	.4
	W	24.5	3.70	.6	24.3	2.42	.4
	E	24.7	2.74	.4	24.	2.51	.4
	S	20.5	2.85	.5	21.6	2.62	.4

TABLE VI.—*Continued.*

RATE: AGE 11.

		Boys, n=36.			Girls, n=35.		
		A	μ	μ_o	A	μ	μ_o
R	F	27.	3.60	.6	27.1	3.44	.6
	W	30.3	4.62	.8	30.4	4.30	.7
	E	29.3	3.41	.6	28.6	3.44	.6
	S	24.1	2.93	.5	24.9	3.30	.6
L	F	23.9	2.57	.4	24.9	3.48	.6
	W	25.9	3.69	.6	26.3	4.02	.7
	E	26.	3.41	.6	26.2	3.57	.6
	S	21.5	2.57	.4	23.6	3.44	.6

RATE: AGE 12.

		Boys, n=33.			Girls, n=34.		
		A	μ	μ_o	A	μ	μ_o
R	F	29.3	5.4	.9	28.2	3.98	.7
	W	31.6	5.27	.9	31.6	5.18	1.0
	E	29.9	4.12	.7	29.4	4.90	.8
	S	25.	3.32	.6	25.7	4.35	.7
L	F	26.3	3.84	.7	25.8	3.44	.6
	W	26.9	3.70	.6	27.	4.90	.8
	E	26.3	3.76	.6	26.2	4.56	.8
	S	22.4	3.59	.6	23.6	3.64	.6

RATE: AGE 13.

		Boys, n=34.			Girls, n=34.		
		A	μ	μ_o	A	μ	μ_o
R	F	28.7	3.49	.6	30.3	4.52	.8
	W	32.3	3.69	.6	33.2	6.03	1.
	E	31.	4.44	.8	30.5	6.20	1.1
	S	25.5	4.52	.8	27.5	4.32	.7
L	F	26.1	3.1	.5	26.7	4.85	.8
	W	27.6	3.72	.6	28.6	4.89	.8
	E	27.5	3.7	.6	28.6	4.15	.7
	S	23.7	3.81	.7	25.2	3.83	.7

TABLE VI.—*Continued.*

RATE: AGE 14.

		Boys, n=41.			Girls, n=33.		
		A	μ	μ_o	A	μ	μ_o
R	F	31.5	3.69	.6	29.5	3.02	.5
	W	33.	3.8	.6	30.3	3.79	.6
	E	32.7	4.63	.7	28.8	2.83	.5
	S	27.2	3.49	.5	26.6	3.17	.5
L	F	[1]27.5	3.69	.6	26.8	2.89	.5
	W	29.	2.61	.4	28.	3.70	.6
	E	28.6	3.62	.6	26.9	3.48	.6
	S	24.3	3.49	.5	23.8	3.34	.6

[1]n=40 in L, F.

RATE: AGE 15.

		Boys, n=32.			Girls, n=31.		
		A	μ	μ_o	A	μ	μ_o
R	F	31.6	4.02	.7	29.1	3.44	.6
	W	34.2	4.46	.8	30.9	4.31	.8
	E	31.5	3.72	.7	29.3	3.84	.7
	S	26.3	3.29	.6	26.	3.89	.7
L	F	28.3	3.46	.6	26.7	3.23	.6
	W	29.5	3.60	.6	28.3	3.97	.7
	E	28.7	3.61	.6	27.6	4.01	.7
	S	24.7	3.58	.6	25.4	4.14	.7

RATE: AGE 16.

		Boys, n=26.			Girls, n=17.		
		A	μ	μ_o	A	μ	μ_o
R	F	33.9	4.92	1.0	31.3	4.66	1.1
	W	35.9	5.16	1.01	30.1	4.93	1.09
	E	32.7	3.90	.8	33.3	4.52	1.2
	S	28.7	3.42	.7	27.9	3.44	.8
L	F	30.7	5.08	1.0	28.6	4.01	1.0
	W	33.1	4.70	.9	29.5	3.87	.9
	E	30.7	2.68	.5	28.2	3.75	.9
	S	26.6	3.52	.7	26.2	3.55	.8

The Degree of Trustworthiness of Results.

I have endeavored to state fully the conditions under which the foregoing tests were made, so that the *a priori* probability of their trustworthiness might be estimated. There are, however, two ways of determining from the results themselves their probable degree of trustworthiness. From whatever known or unknown sources of error the work may have suffered, the net errors are probably within the limits thus ascertained.

1. The mean errors of the means.

Inspection of the tables or charts shows that the limits of mean errors of the means are in all cases absolutely small, and small in comparison with the values of the corresponding means. Further examination shows:

Average of the 160 values of μ_o for ages 6-15, .57 taps in 5″
Sixty-eight per cent. of these values range from 0 to .67 " " "
Maximum value of μ_o, 1.1 " " "

Seventy-seven per cent. of the values of μ_o are less than 2% of their corresponding means.

In sum, if an equal number of individuals, corresponding in age and sex to any class for whom means are here given, be tested under the same conditions, the probability is .683 that the resulting mean will not differ from that here given by more than .67 taps in 5″, and the probability is .978 that the resulting mean will not differ from that here given by more than one tap in 5″.

The relatively small number of individuals of 16 upon whom measurements could be obtained renders the means at that age somewhat less reliable:

Average value of μ_o for age 16, .9 taps in 5″
Maximum value of μ_o, 1.2 " " "
Average $\frac{\mu_o \cdot 100}{\lambda}$ 3% " " "

2. An independent means of judging the degree of trustworthiness of the means is furnished by a comparison of the results from boys with those from girls. It cannot be assumed, and is in fact later shown to be untrue, that the rates for boys and for girls of the same age are approximately equal. On the contrary there appear clearly defined differences varying with the age examined. But if, the fact of characteristic differences between the two sexes having been established, it should appear that the two sets of wholly independent means differ from each other within very narrow limits, then it is probable that two sets of independent means obtained from individuals of the same sex would differ from each other within still narrower limits, certainly not wider ones. If the values of (B-G) for the 88 possible comparisons

(11 ages, 8 joints) between corresponding mean rates of boys and girls be determined, the differences in the mean rates of boys and girls is shown to be very small. Of the 88 values of (B-G),

3 differ by 3 taps or over in 5 seconds.
14 " " 2 " " " " " "
19 " " 1 " " " " " "
52 " " less than 1 tap " " "

In 59 of 88 cases, $(B\text{-}G) < (\mu_o B + \mu_o G)$[1]
That is, in 67% of the cases the difference between the mean rate for boys and the corresponding rate for girls is within the limits of the mean errors of the means compared.

Mean Variation: It will be noted that the mean individual variation is subject to considerable fluctuation. Examination shows that periods of most rapid acceleration are generally periods of widening individual variation, while periods of greatest retardation or decline are periods of narrowing individual variation. The mean individual variation rarely (seven times out of 184) exceeds one tap per second.

Extreme Limits of Variation of Rate.

The lowest mean rate found is 17.9 taps in five seconds (girls left shoulder, 6), the highest mean rate found is 35.9 taps in five seconds (boys, right wrist, 16). If the corresponding mean variation be taken into account, we have 15.5 and 41.2 as the limits within which nearly all individual cases may be expected to fall. Not all, however. In order to show the fact and the degree of probability of extreme high and low rates, I have tabulated the individual results which show rates of less than 4 and of more than 8 per second.[2]

Representing by A the mean rate of a given age; by $A \pm \mu$ the mean with its individual variation; and by N the limits within which 99% of all individual cases fall, we have:

---

[1] $\mu_o B$=mean var. of a mean rate of boys. $\mu_o G$=same for a mean rate of girls.

[2] Very high rates were usually repeated. The best single record was made by a girl, F. W., of 12 in the sixth grade, as follows: R. F., 40; W., 48. (Second trial 47), E., 40; S., 34; L. F., 32; W., 43; E., 40; S., 34.

She looked the type of robust health. When asked if she played the piano she said, "Only by ear; but I play base ball though," adding a moment later, "I can strike two over an octave on the piano." Another of many interesting individual records is that of A. C., a girl of 13, who has taken lessons upon the violin for two years: R. F., 42; W., 43; E., 40; S., 34; L. F., 42; W., 42; E., 34; S., 26.

The high rates of the joints most involved in playing the violin in connection with the low rate of the left shoulder form an interesting picture of the effects of special practice.

	A	A+μ	N
Upper Limit	35.9	41.2	45 taps in 5″
	A	A—μ	N
Lower Limit	17.9	15.5	12 " " "

Of the 5,944 single measurements (including those from five year old children) we have,

Below a rate of 3 per second,	2 cases.
From 3 to 4 per second,	166 "
" 4 " 8 " "	5,709 "
" 8 " 9 " "	61 "
9 per second or over,	6 " [1]

Variation of Rate with Age.

Inspection of the tables or charts shows for both sexes and for all joints an increase of rate with age. The total amount of increase in the rate of each joint in the ten years from 6 to 16 is shown in the following table. A change in decimal point gives the average yearly increase.

Total increase in rate between ages 6 and 16, in terms of x taps in 5″:

Boys' right—F., 14.4; W., 12.9; E., 9.2; S., 8.9.
Boys' left — F., 12.7; W., 13.3; E., 10.3; S., 8.4.
Girls' right—F., 11.5; W., 8.5*; E., 7.4*; S., 8.0.
Girls' left—F., 10.6; W., 10.6; E., 8.5*; S., 8.3.

*Higher at 13 than at 16.

The amount of increase is not, however, the same each year. Table VII. gives the amount of increase or decrease for each joint in each year period. Numbers less than the average increase are printed in italics. Table VIIa. shows the periods of most obvious acceleration and retardation of growth.[2]

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[1] For the behavior of the different joints in successive years, see p. 66.

[2] This table is given not to show the distribution, but the extreme limits of rates found.

TABLE VII.

SHOWING AMOUNT OF INCREASE OR DECREASE IN THE RATE OF EACH JOINT EACH YEAR FROM 6–16 IN TERMS OF X TAPS IN 5."

Ages		6-7	7-8	8-9	9-10	10-11	11-12	12-13	13-14	14-15	15-16
Boys' Right.	F	1.5	2.1	1.3	.8	1.8	2.3	−.6	2.8	.1	2.3
	W	.7	2.6	1.5	.7	1.8	1.3	.7	1.5	.4	1.7
	E	.7	1.9	2.1	−.1	1.2	.6	1.1	1.7	−1.2	1.2
	S	.7	1.8	1.8	−1.5	1.5	.9	.5	1.8	.9	2.5
Boys' Left.	F	1.1	1.4	1.0	.9	1.5	2.3	−.2	1.4	.8	2.4
	W	.5	2.0	1.4	.9	1.4	1.0	.7	1.4	.5	3.6
	E	.5	1.4	1.5	.9	1.3	.3	1.2	1.1	.1	2.0
	S	.6	1.4	.7	.4	1.0	.9	1.3	.6	.4	2.1
Girls' Right.	F	.9	1.5	1.8	1.8	1.3	1.1	2.1	−.8	−.4	2.1
	W	1.5	1.2	1.2	3.0	1.9	1.2	1.6	−2.9	.6	−.8
	E	.5	1.2	1.0	2.1	1.1	.8	.9	−1.7	.5	.8
	S	.3	1.7	.8	−.1	2.3	.8	1.8	−.9	−.6	1.9
Girls' Left.	F	1.1	.6	.9	1.7	2.6	.9	.9	.1	−.1	1.9
	W	1.1	1.0	1.4	1.9	2.0	.7	1.6	−.6	.3	1.2
	E	1.8	.1	1.0	1.4	2.2	.0	2.4	−1.7	.7	.6
	S	.9	1.4	.6	.8	2.0	.0	1.6	−1.4	1.6	.8

TABLE VII a.

Every joint shows an increase greater than the average yearly increase for that joint:

In boys, at ages 7-8, 10-11, 15-16. In girls, 10-11.

Seven of the eight joints show accelerated growth:

In girls, 12-13.

Six of the eight joints show accelerated growth:

In boys, 13-14. In girls, 9-10.

Five of the eight joints show accelerated growth:

In boys, 8-9, 11-12. In girls, 6-7, 7-8, 8-9, 15-16.

Five of the eight joints show an increase of rate less than the average yearly increase for that joint, or a decrease:

In boys, 12-13. In girls, 11-12.

Seven of the eight joints show retardation, or decrease:

In boys, 6-7. In girls, 14-15.

Eight joints show retardation, or decrease:

In boys, 9-10, 14-15. In girls, 13-14.

The degree of acceleration or retardation of growth is shown graphically for each joint each year in the rate chart (I.) by the directions of the solid lines connecting the year-

averages. The plus or minus value of the angle which this line makes with the base line is measured by $\frac{\pm\Delta y}{\Delta t}$, Δt being the constant, 25 mm., and $\pm\Delta y$ being the increase or decrease for the year.

It is obvious from the tables and from the charts that there are certain periods when all or nearly all the joints grow at an accelerated rate, and other periods when all or nearly all grow at a retarded rate or even decline in ability; and it is clear what those periods are. Table VII a. The periods of most considerable and most significant decline are for boys from 14 to 15 and for girls from 13 to 14. It will be observed that in each of these cases the period of decline is preceded by a period of acceleration and is followed by a more or less rapid recovery.[1]

Comparison of Right and Left.

Method of Calculation.

The mean differences between the rates of right side joints and of the corresponding left side joints are obtained from the data in two arithmetically independent ways. (a) The mean rate for a given joint may be compared with that of the corresponding joint on the other side; or (b) the mean of the individual differences between corresponding joints may be ascertained. Both processes must give the same result and they furnish a check against error in calculation.

The values of (r-l) have a higher probability than the values of r or of l.[2] This would be expected a priori, if it were assumed that any degree of correlation exists between corresponding right and left joints. That is, in an entirely new set of individuals a variation in the mean of a right side joint would likely be accompanied by a variation in the mean of the corresponding left side joint, not of precisely the same amount but in the same direction.

That the values of (r-l) *are* more trustworthy than those of r or of l appears from the following consideration: On page 157 it was shown that the mean of the values of μ_0 is .56 taps in 5″ and that two thirds of the values of μ_0 are less than .67 taps in 5.″ If the value of (r-l) be found by the second (b) method described above, the mean variation of individual (r-l) values from the mean (r-l) and the mean variation of the mean (r-l) may be found by the same process as that described on page 151. These calculations were made.[3] The values of

[1] For a comparison of boys and girls in this respect see P. 174. For a discussion of the significance of the facts, see P. 200.

[2] r = mean rate of a right side joint; l = mean rate of a left side joint.

[3] See page 166, where an example of this calculation is given; and pages 167, the results for each pair of joints.

μ_{σ_M}[1] for ages 6 to 15 have an average value of .46 taps in 5"; and two thirds of these values are under .54 taps in 5."

Mean Values of (r-l).

The mean rate of every right side joint is faster than that of the corresponding left side joint; and in every case the plus value of (r-l) is greater than its mean variation. In forty-seven of the 88 cases (11 ages, 4 joints, 2 sexes), (r-l) is greater than the corresponding mean individual variation. The values of each (r-l) may be found from the rate tables, page 153–156. The average values for each joint are as follows:

TABLE VIII.

	Mean of (r-l) values from 6 to 16.	Mean variation of yearly (r-l) values from mean $\frac{\Sigma v.}{n}$	
Boys' Finger	2.8	.68	
" Wrist	4.	.6	
" Elbow	3.4	.58	
" Shoulder	2.2	.53	All values in terms of X taps in 5."
Girls' Finger	2.7	.62	
" Wrist	3.4	.8	
" Elbow	2.5	.58	
" Shoulder	1.7	.8	

Limits of Variation of (r-l).

TABLE IX.

2,992 individual values of (r-l) are distributed in the following proportions:

| | | |

joints, boys and girls). In 91% of these 80 cases the r and l means both show increase or both show decrease. In 85% of the cases the r and l means both show acceleration or both show retardation.

2. The right side joints are subject to greater fluctuation of rate ability than are the left side joints.

Of 68 cases where r and l both show increase, r shows a greater increase than l in 46 cases. Of five cases in which both sides show decrease, r shows a greater decrease than l in four cases. Of the remaining seven cases, r shows a greater plus or minus variation than l in three cases. Of the whole 80 cases, 53 show a greater fluctuation in the right than in the left.

3. Variations in the values of (r-l) with age are dependent of course upon the relative rates of growth in r and l. The rate tables on pages 153–156 and the accompanying chart (Chart II.), exhibit these relations. I find myself only able to say on the whole that the right hand gains faster in certain years, but also loses more in other years, so that at 16 the difference between the two sides is almost the same as at the age of 6.

The total gains of r over l between ages 6 and 16, i. e., (r-l) at 16 minus (r-l) at 6, are as follows:

Boys: F 1.7; W –.4; E –1.1; S .5 taps in 5″ Average .17.
Girls: F .9; W 1.1; E –1.1; S –.3 " " " " .15.

Bilateral Symmetry and Asymmetry of Rate Ability.

It has been shown that the mean rate of right side joints is always greater than that of the corresponding left side joints. It is very desirable to know whether the two sides increase in ability together, and if not, to determine the degree of asymmetry in their development. This does not appear from a comparison of mean rates of right and left side joints in successive years. That widely varying degrees of bilateral correlation might exist in the presence of any given mean rates upon the two sides may be shown by the following illustration:

Let the individuals A, B, C, D and E have the rates 6, 9, 12, 8 and 5 in a right side joint, and the rates 4, 7, 10, 6 and 3 in the corresponding left side joint, it being undetermined which two rates belong to any individual. Assume three distributions of the rates among the individuals as follows:

		R	L	Difference		
I	A	6	4	2	Average R=8. Its	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
	B	9	7	2		
	C	12	10	2	Average L=6. Its	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	D	8	6	2		
	E	5	3	2	Average D=2. Its	$\sqrt{\frac{\Sigma v^2}{n}}=0$
					Average R=8.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
II	A	6	3	3		
	B	9	10	-1	Average L=6.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	C	12	6	6		
	D	8	7	1	Average D=2.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{5.7}$
	E	5	4	1		
					Average R=8.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{6}$
III	A	6	6	0		
	B	9	4	5	Average L=6.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{7.4}$
	C	12	3	9		
	D	8	7	1	Average D=2.	$\sqrt{\frac{\Sigma v^2}{n}}=\sqrt{26.4}$
	E	5	10	-5		

I. is a case of perfect bilateral correlation; II. a case of partial correlation; III. a case of non-correlation.

It is obvious that the value $\sqrt{\frac{\Sigma v^2}{n}}$ obtained from the column of differences between r and l is a measure of the fluctuation of individual (r-l) values from the mean (r-l). In order to determine whether a given value of $\sqrt{\frac{\Sigma v^2}{n}}$ indicates correlation or non-correlation, and what degree of either, it is necessary to determine what the value of $\sqrt{\frac{\Sigma v^2}{n}}$ would be if the relation between right side rates and left side rates *were determined by chance*, i. e., if the r values and l values were placed in separate boxes and paired, by drawing one from each box at a time.

This chance value of $\sqrt{\frac{\Sigma v^2}{n}}$ may be determined as follows :[1]

Let R=mean rate of a right side joint.
(R+r)=any individual rate by that joint.

μ_R=mean individual variation=$\sqrt{\frac{\Sigma v^2}{n}}$

[1]I am indebted for this method and development to Dr. Franz Boas, to whom I hereby return hearty thanks.

L=mean rate of corresponding left side joint.
(L+l)=any individual rate by that joint.
μ_L=corresponding mean individual variation.[1]

(1) Then $P_{(R+r)}=\frac{1}{\mu_R\sqrt{2\pi}}\,\varepsilon-\frac{r^2}{2\mu_R{}^2}$

and $P_{(L+l)}=\frac{1}{\mu_L\sqrt{2\pi}}\,\varepsilon-\frac{l^2}{2\mu_L{}^2}$

are the respective probabilities of any rates (R+r), and (L+l).

If these probabilities are independent of each other, the probability that (R+r) and (L+l) will occur together is:

(2) $P_{(R+r)}\,P_{(L+l)}=\frac{1}{\mu_R\mu_L 2\pi}\,\varepsilon-\frac{r^2}{2\mu_R{}^2}-\frac{l^2}{2\mu_L{}^2}$

(R+r)—(L+l)=(R—L)+(r—l). Substituting u for (r—l).

(3) $P_{R-L+u}=\int_0^\infty\frac{1}{\mu_R\mu_L{}^2 2\pi}\,\varepsilon-\frac{r^2}{2\mu_R{}^2}-\frac{(r-u)^2}{2\mu_L{}^2}$

$$=\frac{1}{\sqrt{2\pi}\,\sqrt{\mu_R{}^2+\mu_L{}^2}}\,\varepsilon-\frac{u^2}{2(\mu_R{}^2+\mu_L{}^2)}$$

It thus appears that if the individual rate (R+r) and the individual rate (L+l) are independent of each other, the mean individual variation in the values of (R+r)—(L+l) will be $\sqrt{\mu_R{}^2+\mu_L{}^2}$ that is, the square root of the sum of the squares of the mean individual variations of (R+r) and (L+l) respectively. The Table XI., page 167, and the Chart III., give the values of μ_o and of $\sqrt{\mu_R{}^2+\mu_L{}^2}$ and of $\frac{\mu_o.100}{\sqrt{\mu_R{}^2+\mu_L{}^2}}$ for each pair of joints at each age from 6 to 16.

The last quantity shows the relation in per cent between the chance value and the actual degree of the bilateral asymmetry. The Table X., page 166, gives an example of the method of determining μ_R (for boys' finger, 6).

Table showing the method of calculating the mean variation of individual values of (r—l), about the average value of (r—l) for boy's finger, 6. Unit=1 tap in 5 seconds.

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[1] For the purposes of this calculation R is used instead of r for the mean rate of a right side joint, and likewise L for l.

TABLE X.

Differences between r and 1 in individual cases=d.	No. of cases of each difference found=n.	dn.	Residuals =v.	v^2	nv^2
-2.	2	-4.	3.3	10.80	21.78
-1.	2	-2.	2.3	5.29	10.58
0.	4	0.	1.3	1.69	6.76
1.	5	5.	.3	.09	.45
2.	8	16.	.7	.49	3.92
3.	2	6.	1.7	2.89	5.78
4.	2	8.	2.7	7.29	14.58
5.	1	5.	3.7	13.69	13.69
	n=26	34.			$\Sigma v^2 = 77.54$

$\frac{34}{26}$=1.3+=Av. (r—1).

$\sqrt{\frac{77.54}{26}}$=1.73-=μ (r—1).

$\frac{1.73}{\sqrt{26}}$=.3+=μ_o (r—1).

TABLE XI.

RATE: BILATERAL SYMMETRY.

Age		Boys. [1] $\sqrt{\mu_R^2+\mu_L^2}$	Boys. [2] $\mu_B^{(2)}$	Boys. [3] $\frac{\mu_B .100}{\sqrt{\mu_R^2+\mu_L^2}}$ %	Girls. $\sqrt{\mu_R^2+\mu_L^2}$	Girls. $\mu_B^{(2)}$	Girls. $\frac{\mu_B .100}{\sqrt{\mu_R^2+\mu_L^2}}$ %
6	F	2.41	1.73	71.8	3.75	2.5	45.3
	W	3.55	2.43	68.4	3.47	2.25	64.8
	E	3.13	2.34	74.7	3.01	1.78	59.1
	S	3.62	2.58	71.2	3.61	2.43	67.3
7	F	3.5	2.57	73.4	4.06	3.35	82.5
	W	3.9	2.71	69.4	3.68	2.44	66.3
	E	4.83	2.54	52.5	3.61	1.78	49.3
	S	3.54	2.34	66.1	3.82	2.27	59.4
8	F	3.28	2.12	64.3	3.82	2.18	57.
	W	4.03	2.53	62.7	3.82	2.39	62.5
	E	3.68	2.40	65.2	3.55	1.84	51.8
	S	3.42	2.41	70.4	4.11	2.01	48.9
9	F	5.46	2.92	53.4	4.04	2.69	66.5
	W	5.17	3.36	64.9	4.39	2.55	58
	E	5.09	3.18	62.4	4.68	2.95	63.
	S	5.09	2.80	55.	4.83	2.55	52.7
10	F	3.86	1.62	41.9	4.28	2.99	69.8
	W	4.96	3.06	61.7	4.41	3.20	72.5
	E	4.5	2.76	61.3	3.91	2.78	71.
	S	3.89	2.39	61.4	3.82	2.19	57.3
11	F	4.44	2.59	58.3	4.88	2.84	58.1
	W	5.9	3.56	60.2	5.87	3.54	60.3
	E	4.81	2.92	60.7	4.95	3.26	65.8
	S	3.89	3.09	79.4	4.74	2.63	55.4
12	F	6.6	4.43	67.1	5.25	2.68	51.
	W	6.38	3.62	56.7	7.	3.08	44.
	E	5.52	3.09	55.9	6.72	2.64	30.3
	S	4.88	2.78	56.9	5.61	2.77	49.3
13	F	4.67	3.49	74.7	6.58	3.14	47.7
	W	5.16	3.32	64.3	7.75	3.46	44.6
	E	5.75	3.23	57.9	7.27	3.52	48.4
	S	5.89	2.53	42.9	5.51	3.01	56.4
14	F	5.23	3.33	63.6	4.17	3.33	58.7
	W	4.64	2.89	62.2	5.3	2.89	57.8
	E	5.84	4.07	69.6	4.48	3.50	78.1
	S	4.95	3.21	65.	4.59	3.40	74.2
15	F	5.31	3.94	74.1	4.67	2.91	62.3
	W	5.76	3.65	63.1	5.87	3.34	56.8
	E	5.16	2.78	55.8	5.51	2.42	43.9
	S	4.88	2.94	60.2	5.59	2.41	43.1
16	F	7.08	4.16	58.7	6.17	2.61	42.3
	W	7.01	4.32	61.6	6.26	2.13	34.
	E	4.86	2.94	60.4	5.83	3.66	62.7
	S	4.88	2.87	58.8	4.88	3.04	62.2

[1] This column is the *solid upper* line in bilateral charts. (III.)
[2] This column is the *dotted lower* line in bilateral charts.
[3] This column shows per cent. of actual bilateral asymmetry to its chance value.

The fact of a certain degree of bilateral asymmetry and of a certain degree of bilateral symmetry, and the degree of each, are thus shown for each pair of joints at each age.

Bilateral Symmetry and Asymmetry of Development.

If it should appear that μ_B changes from year to year in the same direction and in an approximately corresponding degree with $\sqrt{\mu_R^2+\mu_L^2}$ then it would be inferred that the causes which determine the fluctuations of individual variation determine also the fluctuations of bilateral asymmetry; otherwise stated, that corresponding right and left side joints in most individuals do not grow together, do not have corresponding acceleration and retardation of growth together. Tendency of the curve μ_B to move parallel with the curves $\sqrt{\mu_R^2+\mu_L^2}$ is so far proof of asymmetrical growth.

On the other hand, if it should appear that μ_B altogether refuses to fluctuate with $\sqrt{\mu_R^2+\mu_L^2}$ then it would be inferred that the causes which bring about fluctuations of individual variations do not affect the degree of bilateral asymmetry; otherwise stated, that corresponding right and left side joints in most individuals grow together; have corresponding acceleration and retardation of growth together, through whatever periods of individual variation. Tendency of the curve μ_B to move independently in direction of the curve $\sqrt{\mu_R^2+\mu_L^2}$ is so far proof of symmetrical growth.

If we determine the amount of yearly increase or decrease in the values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$ for each joint, we have in each case 80 results, 40 for boys and 40 for girls. A comparison of the 80 plus or minus increments of μ_B with the 80 plus or minus increments of $\sqrt{\mu_R^2+\mu_L^2}$ gives the following results:

I. μ_B and $\sqrt{\mu_R^2+\mu_L^2}$ tend to vary in the same direction oftener than would be accounted for by chance. In 35 out of 80 comparisons, both increase; in 15 out of 80 comparisons, both decrease; i. e., in 50 out of 80 comparisons, both change in the same direction. There is, therefore, so far, bilateral asymmetry of development.

The probability from the totals is 62.5 per cent that the degree of bilateral asymmetry will be changed to some extent in the same direction as the given function of the individual variations. For finger, wrist and elbow, the probability is greater, viz.: 70%, or 20% more than an even chance.

2. Besides the 30 cases out of the total 80 in which μ_B and $\sqrt{\mu_R+\mu_L^2}$ do not vary in the same direction, there are 38 cases in which the latter varies more than the former. There

is, therefore, so far, symmetry of development of the two sides.

The probability is 72.5% that the given function of the individual variation will fluctuate more than the bilateral asymmetry; and the probability is 85% that the mean variation will fluctuate more than the bilateral asymmetry or will change in the opposite direction. The probability is 15%, or 35% less than an even chance, that in any given case the bilateral asymmetry will vary in the same direction and in a greater degree than the given function of the mean variation.

The following table gives the mean values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$ for each joint of boys and of girls, together with the corresponding $\left(\frac{\Sigma v}{n}\right)$ fluctuation of yearly values from the mean. In each case the fluctuation is shown to be much greater for $\sqrt{\mu_R^2+\mu_L^2}$ than for μ_B.

TABLE XII.

		Finger	$\frac{\Sigma v}{n}$	Wrist	$\frac{\Sigma v}{n}$	Elbow	$\frac{\Sigma v}{n}$	Shoulder	$\frac{\Sigma v}{n}$
$\sqrt{\mu_R^2+\mu_L^2}$	Boys	4.53	1.13	5.13	.85	4.74	53.	4.44	.70
	Girls	4.60	.73	5.25	1.15	4.86	1.08	4.64	.59
μ_B	Boys	2.95	.79	3.22	.45	3.02	.36	2.72	.25
	Girls	2.86	.27	2.93	.45	2.79	.54	2.52	.32

The fact and the degree of each tendency are shown in the graphical representation of each pair of joints by the relative directions of the lines connecting successive values of μ_B and of $\sqrt{\mu_R^2+\mu_L^2}$. The tendency of the two curves to run parallel and the greater fluctuation of the upper curve are both evident. It is, therefore, to be concluded that there is partial, and only partial, bilateral asymmetry in the development of the rate of voluntary movement. Corresponding joints do not have exactly corresponding growth, but the correspondence is considerably closer than would be accounted for by chance.

COMPARISON OF THE JOINTS.

The order of the joints as regards rate is not the same at all ages. The following table shows the order at each age:

TABLE XIII.

Order		1	2	3	4	Order		1	2	3	4
6	E	4	0	0	0	7	E	4	0	0	0
	W	0	4	0	0		W	0	4	0	0
	S	0	0	3	1		F	0	0	4	0
	F	0	0	1	3		S	0	0	0	4
8[1]	E	3	0	0	0	9	E	3	1	0	0
	W	0	3	0	0		W	1	3	0	0
	F	0	0	3	1		F	0	0	3	1
	S	0	0	1	3		S	0	0	1	3
10	W	4	0	0	0	11	W	3	1	0	0
	E	0	4	0	0		E	1	3	0	0
	F	0	0	4	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
12[2]	W	4	0	0	0	13	W	3	1	0	0
	E	0	3	0	0		E	1	3	0	0
	F	0	0	3	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
14	W	4	0	0	0	15	W	4	0	0	0
	E	0	3	1	0		E	0	3	1	0
	F	0	1	3	0		F	0	1	3	0
	S	0	0	0	4		S	0	0	0	4
16[3]	W	4	0	0	0						
	F	0	3	0	0						
	E	0	0	3	0						
	S	0	0	0	4						

[1] E=W1. [2] E=F. [3] E=F.

The figures show the number of times, out of a possible 4, that the given joint stands in the order indicated. Thus at the age of 6, the mean rate of the elbow is highest in four cases,— boys R and L, and girls R and L.

TABLE XIV.

Table showing the number of times, out of a possible 44, each joint is found in each order as regards rate.

	Highest	Second	Third	Lowest	
Wrist	27	16	0	0	= 43
Elbow	16	20	5	0	= 41
Finger	0	5	32	5	= 42
Shoulder	0	0	5	39	= 44

Once E=W. Twice E=F.

The following table shows the amount by which elbow, wrist and finger severally exceed the shoulder at each age:

TABLE XV.

		Boys.			Girls.		
	Years	E-S	W-S	F-S	E-S	W-S	F-S
Right	6	3.7	3.4	-.3	2.8	1.7	-.1
	7	3.7	3.2	.5	3.0	2.9	.5
	8	3.8	3.8	.8	2.5	2.4	.3
	9	4.1	3.7	.3	2.7	3.2	1.3
	10	6.5	5.9	2.6	4.9	5.9	3.2
	11	5.6	6.4	2.9	3.7	5.5	2.2
	12	4.9	6.6	4.3	3.7	5.9	2.5
	13	5.6	6.8	3.2	3.7	5.7	2.8
	14	5.5	5.8	4.3	3.0	3.7	2.9
	15	4.9	7.9	5.3	2.2	4.9	3.1
	16	4.0	7.3	5.3	3.1	5.4	3.4
Left	6	2.2	1.5	-.2	1.8	1.0	.1
	7	2.1	1.4	.3	2.7	1.2	.3
	8	2.1	2.0	.3	1.4	.8	-.5
	9	2.9	2.7	.6	1.8	1.8	-.2
	10	4.2	4.2	1.9	2.4	2.5	.7
	11	4.5	4.4	2.4	2.6	2.7	1.3
	12	3.9	4.5	3.9	2.6	3.4	2.2
	13	3.8	3.9	2.4	3.4	3.4	1.5
	14	4.3	4.7	3.2	3.1	4.2	3.0
	15	4.0	5.1	3.8	2.2	2.8	1.3
	16	4.1	6.4	4.1	2.0	3.3	2.4

These results show that the shoulder grows most slowly, the elbow slightly faster, the wrist and finger very much more

rapidly. At 6, the finger joint is slowest, the elbow fastest; at 16, the finger has passed the elbow.[1]

Longitudinal Asymmetry.

It is possible, by the method employed in determining the fact and degree of bilateral asymmetry, to determine whether there is any degree, and if so, what degree, of longitudinal asymmetry. The results for the right side in boys of 6, 9, 12 and 15 were treated in this manner, the remaining material being reserved for future treatment. The following table shows a comparison of the values of $\mu_{Long.}$ with the values of $\sqrt{\mu_{J1}^2+\mu_{J2}^2}$ for the ages named, finger and wrist, wrist and elbow, elbow and shoulder; finger and shoulder being so compared.

μ_J = mean variation of one of the joints compared.
$\mu_{Long.}$ = mean variation of individual values of (J_1-J_2) from their mean.

Table XVI.

Table Showing Longitudinal Asymmetry.

Joints Compared	Ages	I $\sqrt{\mu_{J1}^2+\mu_{J2}^2}$	II Long. Asymmetry $\mu_{Long.}$	III $\frac{\text{II. } 100}{\text{I.}}$ = % of Asymmetry
Finger and Wrist	6	3.25	2.15	.66
	9	5.43	2.94	.49
	12	7.61	4.05	.53
	15	6.	3.89	.65
Wrist and Elbow	6	3.08	2.72	.88
	9	4.97	2.52	.50
	12	6.69	3.50	.52
	15	5.81	4.02	.69
Elbow and Shoulder	6	3.16	3.25	1.02
	9	5.07	2.54	.50
	12	5.29	3.52	.66
	15	4.97	3.49	.70
Finger and Shoulder	6	3.33	3.15	.94
	9	5.54	4.21	.76
	12	6.34	4.04	.63
	15	5.19	4.36	.84

[1]For discussion, see page 201.

The fact of partial and only partial asymmetry of development, is thus shown to hold longitudinally as well as bilaterally. It will be observed that the degree of asymmetry approaches most nearly its chance value at age 6, and that in one instance at that age it is slightly in excess of its chance value. As in the bilateral comparisons, however, the absolute degree of asymmetry does not change much with age. The greatest asymmetry, absolute and relative, appears between finger and shoulder; the greatest correlation, between finger and wrist.

Boys and Girls.

It was shown on page 50 that the differences between boys and girls are slight. Within the narrow limits there indicated, however, there is a slight superiority of boys over girls. Of the 29 cases in which the difference between boys and girls is greater than $(\mu_{cB}+\mu_{cG})$,[1] 24 are in favor of the boys and 5 in favor of the girls. Of the remaining 59 cases, 33 are in favor of boys, 19 in favor of girls and 7 are the same in both. The superiority of the boys over the girls increases slightly from the age of 6 to the age of 9; and more decidedly from 14 to 16. They are nearest together at 10, 11 and 12. At 13 the girls are superior to the boys for each of the eight joints tested. It has been pointed out elsewhere that the period from 12 to 13 is a period of retardation of rate in boys and acceleration in girls. (Page 160, Table VIIa.)

The superiority of the boys' right side over the girls' right side is slightly greater than the superiority of the boys' left over the girls' left. On the right side, 16 out of 44 cases show $(B—G) > (\mu_{cB}+\mu_{cG})$. Upon the left side there are eight such cases out of 44.

From this fact it comes that there is a slightly greater difference between the right and left sides in boys than in girls. In 33 out of 44 cases [(r-l) boys compared with (r-l) girls] the difference between the right and left is greater for boys than for girls. For each joint the average difference between r and l is greater for boys than for girls. See page 162, Table VIII.[2]

Further light is thrown upon these relations by a study of the increase of rate in boys and girls. If the amount of in-

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[1] μ_{cB} = the mean variation of a mean rate made by boys.
μ_{cG} = " " " " " " " " " girls.

[2] The value of (r-l) boys — (r-l) girls is small, as follows:
2 taps or over in 5 seconds, 2 cases.
Between 1 and 2 taps in 5 seconds, 15 cases.
Less than 1 tap in 5 seconds, 27 cases.

crease in rate for each year be found for each joint tested, the following facts appear:

On the right side the amount of yearly increase of rate is greater for boys than for girls in 24 out of 40 cases. On the left side the boys' rate increases more than that of girls in 20 out of 40 cases. This indicates that on the whole, the boys' right side improves slightly faster than the girls' right side, while the boys' left side improves no faster, possibly more slowly, than the girls' left side.

Bilateral Asymmetry in Boys and Girls.

Above it has been shown that the absolute difference between right and left is greater for boys than for girls, due to the more rapid development of the right side in boys. A study of the mean variation about the average (r–l) shows that there is a closer bilateral correlation in girls than in boys. If we represent by μ_B and μ_G the values of the mean variations about the average (r–l) values for each joint tested, of boys and girls respectively, a table of $(\mu_B - \mu_G)$ values shows the following results:

1. The value of $(\mu_B - \mu_G)$ is small, as follows:
In 2 cases out of 44, 2 taps or more in 5 seconds.
" 12 " " " " 1 " " " " " "
" 30 " " " " less than 1 tap in " "
" 31 " " " " the value of $(\mu_B - \mu_G)$ is less than the sum of the mean errors of the mean (r–l) values concerned.

2. In 12 cases out of 44 $(\mu_B - \mu_G) > (\mu_{oB} + \mu_{oG})$ and is plus, i. e., the asymmetry is greater for boys than for girls.
In 1 case out of 44 $(\mu_B - \mu_G) > (\mu_{oB} + \mu_{oG})$ and is minus i. e., the asymmetry is greater for girls than for boys.
In 19 of the 31 cases in which $(\mu_B - \mu_G) < (\mu_{oB} + \mu_{oG})$, the value of $(\mu_B - \mu_G)$ is plus. In 10 cases this value is minus. In 2 cases it is 0. In the case of every joint, the average bilateral asymmetry is greater for boys than for girls.

This greater bilateral asymmetry in boys is affected by the varying rapidities of growth in the two sexes. For example, from 9 to 10 and from 12 to 13 are clearly marked periods of retarded growth of rate for boys. In these years, the difference in symmetry between boys and girls is reduced practically to zero. In the years from 7 to 9, or from 15 to 16, on the contrary, especially in the latter period, the greater bilateral asymmetry of boys is clear.

It was pointed out (Page 161) that there is a decline of rate in girls from 13 to 14, and in boys from 14 to 15, that these periods are preceded by a year of accelerated growth, and are followed by more or less rapid recovery. It is significant

that the decline and the antecedent acceleration are more extreme in girls, and that the recovery is slower. In proof:

A comparison of the rates of girls at 13 with the rates of girls at 16, shows that the former almost reach and in three cases surpass the latter.

Fifteen of the twenty-five individual rates of 8 per second or over, made by girls, were made by girls of 13.

Although, as shown elsewhere, the rate of girls is generally slightly less than that of boys,—at the age of 13, every joint shows a higher average in girls than in boys; and in the case of four joints, the girlsof 13 are faster than the boys of 14.

The decline is greater in the case of girls.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

The girls recover more slowly.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

Comparison of the increments of rate in boys from 15 to 16 with those in girls from 14 to 15, shows the former to be decidedly greater in the case of every joint; and in the case of seven of the eight joints, the increment of rate in boys from 15 to 16 is greater than that in girls from 14 to 16.

Note on Results from Left-handed Children.

The small number of left-handed subjects at any one age prevents much profitable comparison of these records with those from right-handed subjects. In 11 out of 80 (10 yrs., 4 joints, 2 sexes) cases (14%), the mean rate of right-hand joints is greater in left-handed than in right-handed subjects; in 55 out of 80 cases (69%), the left-hand joints of left-handed subjects are faster than the corresponding joints of right-handed subjects; in 66 out of 160 cases (41.2%), the mean rate of joints in left-handed subjects is faster than that of the corresponding joints in right-handed subjects. These percentages are only to be taken as rough approximations.

The fact that values of (r-l) do not fluctuate so much at different ages as to make them incomparable, has caused me to calculate the mean (r-l) for each joint of the 26 boys and of the 20 girls irrespective of age. The results (Table XVII.) show that the average difference between r and l is very small compared with the difference in right-handed subjects, and that notwithstanding the heterogeneity of age the bilateral asymmetry is generally smaller than in right-handed subjects.

TABLE XIII.

Order		1	2	3	4	Order		1	2	3	4
6	E	4	0	0	0	7	E	4	0	0	0
	W	0	4	0	0		W	0	4	0	0
	S	0	0	3	1		F	0	0	4	0
	F	0	0	1	3		S	0	0	0	4
8[1]	E	3	0	0	0	9	E	3	1	0	0
	W	0	3	0	0		W	1	3	0	0
	F	0	0	3	1		F	0	0	3	1
	S	0	0	1	3		S	0	0	1	3
10	W	4	0	0	0	11	W	3	1	0	0
	E	0	4	0	0		E	1	3	0	0
	F	0	0	4	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
12[2]	W	4	0	0	0	13	W	3	1	0	0
	E	0	3	0	0		E	1	3	0	0
	F	0	0	3	0		F	0	0	4	0
	S	0	0	0	4		S	0	0	0	4
14	W	4	0	0	0	15	W	4	0	0	0
	E	0	3	1	0		E	0	3	1	0
	F	0	1	3	0		F	0	1	3	0
	S	0	0	0	4		S	0	0	0	4
16[3]	W	4	0	0	0						
	F	0	3	0	0						
	E	0	0	3	0						
	S	0	0	0	4						

[1] E=W1. [2] E=F. [3] E=F.

The figures show the number of times, out of a possible 4, that the given joint stands in the order indicated. Thus at the age of 6, the mean rate of the elbow is highest in four cases,— boys R and L, and girls R and L.

TABLE XIV.

Table showing the number of times, out of a possible 44, each joint is found in each order as regards rate.

	Highest	Second	Third	Lowest	
Wrist	27	16	0	0	= 43
Elbow	16	20	5	0	= 41
Finger	0	5	32	5	= 42
Shoulder	0	0	5	39	= 44

Once E=W. Twice E=F.

The following table shows the amount by which elbow, wrist and finger severally exceed the shoulder at each age:

TABLE XV.

	Boys.				Girls.		
	Years	E-S	W-S	F-S	E-S	W-S	F-S
Right	6	3.7	3.4	-.3	2.8	1.7	-.1
	7	3.7	3.2	.5	3.0	2.9	.5
	8	3.8	3.8	.8	2.5	2.4	.3
	9	4.1	3.7	.3	2.7	3.2	1.3
	10	6.5	5.9	2.6	4.9	5.9	3.2
	11	5.6	6.4	2.9	3.7	5.5	2.2
	12	4.9	6.6	4.3	3.7	5,9	2.5
	13	5.6	6.8	3.2	3.7	5.7	2.8
	14	5.5	5.8	4.3	3.0	3.7	2.9
	15	4.9	7.9	5.3	2.2	4.9	3.1
	16	4.0	7.3	5.3	3.1	5.4	3.4
Left	6	2.2	1.5	-.2	1.8	1.0	.1
	7	2.1	1.4	.3	2.7	1.2	.3
	8	2.1	2.0	.3	1.4	.8	-.5
	9	2.9	2.7	.6	1.8	1.8	-.2
	10	4.2	4.2	1.9	2.4	2.5	.7
	11	4.5	4.4	2.4	2.6	2.7	1.3
	12	3.9	4.5	3.9	2.6	3.4	2.2
	13	3.8	3.9	2.4	3.4	3.4	1.5
	14	4.3	4.7	3.2	3.1	4.2	3.0
	15	4.0	5.1	3.8	2.2	2.8	1.3
	16	4.1	6.4	4.1	2.0	3.3	2.4

These results show that the shoulder grows most slowly, the elbow slightly faster, the wrist and finger very much more

rapidly. At 6, the finger joint is slowest, the elbow fastest; at 16, the finger has passed the elbow.[1]

Longitudinal Asymmetry.

It is possible, by the method employed in determining the fact and degree of bilateral asymmetry, to determine whether there is any degree, and if so, what degree, of longitudinal asymmetry. The results for the right side in boys of 6, 9, 12 and 15 were treated in this manner, the remaining material being reserved for future treatment. The following table shows a comparison of the values of $\mu_{Long.}$ with the values of $\sqrt{\mu_{J1}^2+\mu_{J2}^2}$ for the ages named, finger and wrist, wrist and elbow, elbow and shoulder; finger and shoulder being so compared.

μ_J = mean variation of one of the joints compared.
$\mu_{Long.}$ = mean variation of individual values of (J_1-J_2) from their mean.

Table XVI.

Table Showing Longitudinal Asymmetry.

Joints Compared	Ages	I $\sqrt{\mu_{J1}^2+\mu_{J2}^2}$	II Long. Asymmetry $\mu_{Long.}$	III $\frac{\text{II. } 100}{\text{I.}}$ = % of Asymmetry
Finger and Wrist	6	3.25	2.15	.66
	9	5.43	2.94	.49
	12	7.61	4.05	.53
	15	6.	3.89	.65
Wrist and Elbow	6	3.08	2.72	.88
	9	4.97	2.52	.50
	12	6.69	3.50	.52
	15	5.81	4.02	.69
Elbow and Shoulder	6	3.16	3.25	1.02
	9	5.07	2.54	.50
	12	5.29	3.52	.66
	15	4.97	3.49	.70
Finger and Shoulder	6	3.33	3.15	.94
	9	5.54	4.21	.76
	12	6.34	4.04	.63
	15	5.19	4.36	.84

[1]For discussion, see page 201.

The fact of partial and only partial asymmetry of development, is thus shown to hold longitudinally as well as bilaterally. It will be observed that the degree of asymmetry approaches most nearly its chance value at age 6, and that in one instance at that age it is slightly in excess of its chance value. As in the bilateral comparisons, however, the absolute degree of asymmetry does not change much with age. The greatest asymmetry, absolute and relative, appears between finger and shoulder; the greatest correlation, between finger and wrist.

Boys and Girls.

It was shown on page 50 that the differences between boys and girls are slight. Within the narrow limits there indicated, however, there is a slight superiority of boys over girls. Of the 29 cases in which the difference between boys and girls is greater than $(\mu_{cB}+\mu_{cG})$,[1] 24 are in favor of the boys and 5 in favor of the girls. Of the remaining 59 cases, 33 are in favor of boys, 19 in favor of girls and 7 are the same in both. The superiority of the boys over the girls increases slightly from the age of 6 to the age of 9; and more decidedly from 14 to 16. They are nearest together at 10, 11 and 12. At 13 the girls are superior to the boys for each of the eight joints tested. It has been pointed out elsewhere that the period from 12 to 13 is a period of retardation of rate in boys and acceleration in girls. (Page 160, Table VIIa.)

The superiority of the boys' right side over the girls' right side is slightly greater than the superiority of the boys' left over the girls' left. On the right side, 16 out of 44 cases show $(B—G) > (\mu_{cB}+\mu_{cG})$. Upon the left side there are eight such cases out of 44.

From this fact it comes that there is a slightly greater difference between the right and left sides in boys than in girls. In 33 out of 44 cases [(r-l) boys compared with (r-l) girls] the difference between the right and left is greater for boys than for girls. For each joint the average difference between r and l is greater for boys than for girls. See page 162, Table VIII.[2]

Further light is thrown upon these relations by a study of the increase of rate in boys and girls. If the amount of in-

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[1] μ_{cB} = the mean variation of a mean rate made by boys.
μ_{cG} = " " " " " " " " girls.

[2] The value of (r-l) boys — (r-l) girls is small, as follows:
2 taps or over in 5 seconds, 2 cases.
Between 1 and 2 taps in 5 seconds, 15 cases.
Less than 1 tap in 5 seconds, 27 cases.

crease in rate for each year be found for each joint tested, the following facts appear:

On the right side the amount of yearly increase of rate is greater for boys than for girls in 24 out of 40 cases. On the left side the boys' rate increases more than that of girls in 20 out of 40 cases. This indicates that on the whole, the boys' right side improves slightly faster than the girls' right side, while the boys' left side improves no faster, possibly more slowly, than the girls' left side.

Bilateral Asymmetry in Boys and Girls.

Above it has been shown that the absolute difference between right and left is greater for boys than for girls, due to the more rapid development of the right side in boys. A study of the mean variation about the average (r–l) shows that there is a closer bilateral correlation in girls than in boys. If we represent by μ_b and μ_g the values of the mean variations about the average (r–l) values for each joint tested, of boys and girls respectively, a table of $(\mu_b - \mu_g)$ values shows the following results:

1. The value of $(\mu_b - \mu_g)$ is small, as follows:
In 2 cases out of 44, 2 taps or more in 5 seconds.
" 12 " " " " 1 " " " " " "
" 30 " " " " less than 1 tap in " "
" 31 " " " " the value of $(\mu_b - \mu_g)$ is less than the sum of the mean errors of the mean (r–l) values concerned.

2. In 12 cases out of 44 $(\mu_b - \mu_g) > (\mu_{ob} + \mu_{og})$ and is plus, i. e., the asymmetry is greater for boys than for girls.

In 1 case out of 44 $(\mu_b - \mu_g) > (\mu_{ob} + \mu_{og})$ and is minus i. e., the asymmetry is greater for girls than for boys.

In 19 of the 31 cases in which $(\mu_b - \mu_g) < (\mu_{ob} + \mu_{og})$, the value of $(\mu_b - \mu_g)$ is plus. In 10 cases this value is minus. In 2 cases it is 0. In the case of every joint, the average bilateral asymmetry is greater for boys than for girls.

This greater bilateral asymmetry in boys is affected by the varying rapidities of growth in the two sexes. For example, from 9 to 10 and from 12 to 13 are clearly marked periods of retarded growth of rate for boys. In these years, the difference in symmetry between boys and girls is reduced practically to zero. In the years from 7 to 9, or from 15 to 16, on the contrary, especially in the latter period, the greater bilateral asymmetry of boys is clear.

It was pointed out (Page 161) that there is a decline of rate in girls from 13 to 14, and in boys from 14 to 15, that these periods are preceded by a year of accelerated growth, and are followed by more or less rapid recovery. It is significant

that the decline and the antecedent acceleration are more extreme in girls, and that the recovery is slower. In proof:

A comparison of the rates of girls at 13 with the rates of girls at 16, shows that the former almost reach and in three cases surpass the latter.

Fifteen of the twenty-five individual rates of 8 per second or over, made by girls, were made by girls of 13.

Although, as shown elsewhere, the rate of girls is generally slightly less than that of boys,—at the age of 13, every joint shows a higher average in girls than in boys; and in the case of four joints, the girlsof 13 are faster than the boys of 14.

The decline is greater in the case of girls.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

The girls recover more slowly.

Comparison of the retardation of rate in boys from 14 to 15 with that in girls from 13 to 14, shows the latter to be greater in the case of seven of the eight joints. The same facts appear graphically in the rate charts.

Comparison of the increments of rate in boys from 15 to 16 with those in girls from 14 to 15, shows the former to be decidedly greater in the case of every joint; and in the case of seven of the eight joints, the increment of rate in boys from 15 to 16 is greater than that in girls from 14 to 16.

Note on Results from Left-handed Children.

The small number of left-handed subjects at any one age prevents much profitable comparison of these records with those from right-handed subjects. In 11 out of 80 (10 yrs., 4 joints, 2 sexes) cases (14%), the mean rate of right-hand joints is greater in left-handed than in right-handed subjects; in 55 out of 80 cases (69%), the left-hand joints of left-handed subjects are faster than the corresponding joints of right-handed subjects; in 66 out of 160 cases (41.2%), the mean rate of joints in left-handed subjects is faster than that of the corresponding joints in right-handed subjects. These percentages are only to be taken as rough approximations.

The fact that values of (r-l) do not fluctuate so much at different ages as to make them incomparable, has caused me to calculate the mean (r-l) for each joint of the 26 boys and of the 20 girls irrespective of age. The results (Table XVII.) show that the average difference between r and l is very small compared with the difference in right-handed subjects, and that notwithstanding the heterogeneity of age the bilateral asymmetry is generally smaller than in right-handed subjects.

XVII.

Table showing mean values of (r-l) in left-handed subjects and degree of bilateral asymmetry.

	Av. (r-l)	$\frac{\Sigma v}{n}$		Av. (r-l)	$\frac{\Sigma v}{n}$
Boys F	1.2	3.3	Girls	—.2	2.4
W	—.1	2.7		.1	3.
E	—.8	2.1		.8	3.1
S	—.2	2.1		—.2	.95

REVIEW OF FACTS ON RATE OF VOLUNTARY MOVEMENT.

1. The maximum rate of rhythmically repeated voluntary movement is subject to changes in a given individual which are usually slight and gradual. (P. 142.)

2. These changes are sufficient in amount and in constancy to indicate, surely, local and general subjective conditions, as excitement, general and local fatigue, local cold, and the improvement with age. (P. 144 *et seq.*)

3. The change of rate with extreme fatigue is large in comparison with the mean rate of improvement with age. (P. 148.)

4. The amplitude of movement may be changed within wide limits without affecting the rate. (P. 150.)

5. The mean rate of growth of rate between ages 6 and 16 ranges from .15 to .3 taps per second in various joints. (P. 159.)

6. The rate of growth of rate ability is not uniform. Well marked periods of accelerated and of retarded growth appear. (P. 160.)

7. The mean rate of a right side joint, for a group of right-handed subjects, is always higher than that of the corresponding left side joint. The probability that the right will exceed the left in any case chosen at random is about 80% (P. 161 *et seq.*)

8. The mean rates of corresponding right and left side joints both increase or both decrease in about 90% of cases.

9. Right side joints are subject to slightly greater plus and minus fluctuations of rate ability than are left side joints. (P. 162, 163.)

10. Right side joints gain little if any in rate ability, more than do left side joints. (P. 163.)

11. There is partial and only partial asymmetry of development, bilateral and longitudinal. (P. 163–169, 172.)

12. The hand outgrows the arm between the ages here examined. (P. 170 *et seq.*)

13. The mean rate of boys slightly exceeds that of girls at all ages, except where retardation of growth in boys coincides with acceleration of growth in girls. (P. 173.)

14. There is less bilateral asymmetry of development in the rate ability of boys than in that of girls. (P. 174.)

15. The left-handed persons examined show decidedly less mean difference between right and left and less bilateral asymmetry than do right-handed persons. (P. 175.)

Precision of Voluntary Movement as Regards Force and Direction.

I.

There is no lack of sufficiently delicate qualitative tests of precision of movement. Personal carriage, speech, games, industrial occupations, scientific technique, fine arts,—in short, all forms of active life afford a multitude of such tests, by means of which the degree of muscular control or lack of it is more or less accurately estimated.

For the clinical determination of precision of movement, besides taking notice of visible irregularities of muscular control and of irregularities shown in any of the subject's ordinary work or play, the following special devices have been used. The patient tries:

1. To draw a straight line ([13]).
2. To write his name or other words ([14]), ([15]).
3. To touch suddenly a specified spot with the point of a pencil ([16]).
4. To hold a reed attached to the finger still, in position to write upon, ([13]) or to cast a shadow upon, ([17]) the revolving drum.
5. To apply constant pressure to some form of dynamograph ([18]), ([19]), ([20]).

All these devices test the control of amount of force exerted, the dynamograph doubtless best. All except the dynamograph test also particularly the control of the direction in which force is exerted. All give or may furnish material for a graphic record of results; and it is not impossible to work out from any of them, with sufficient labor, a numerical result. It is doubtful whether in practice any one seeks to get a numerical result from any of these devices, except the third, and in that case "the result is hardly worth the trouble." (Gowers I. 5.)

II.

The idea has presented itself that precision of movement as dependent upon control of the amount and direction of force may be accurately and conveniently measured, giving a numerical and if desired a graphic result, by a variety of devices the essential point of which is as follows: To one pole of a battery is attached an apparatus which presents a series of spaces, graded in size as finely as desired, and bounded by the conducting medium; to the opposite pole is attached some appropriate form and size of stylus. Or, the stylus may vary in size, the open space in the other electrode being of some appropriate form and size. The task in either case is to determine within what limits of precision either or both of the electrodes may be moved or held still without making contact. The numerical result is read from the instrument. A graphic result can alway be readily constructed from the numerical, and in some forms of apparatus to be described, may be made by the subject.

III.

Five forms of apparatus were made upon this principle, adapted to test various muscles and movements. In the experiments here reported, two of these forms were used. The first of these, Fig. 2, is essentially a device for measuring the precision shown in drawing a straight line. Upon a smooth and hard surface (A) (e. g., glass) were fastened two strips of platinum-foil (B) so that they formed an acute angle 2°-2½°

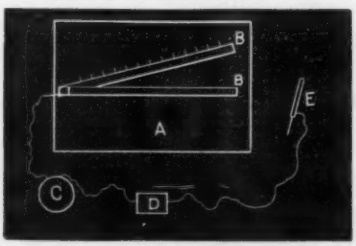

FIG. 2.

A—Plate Glass. *B*—Strips of foil. *C*—Battery. *D*—Sounder. *E*—Stylus.

with each other. The platinum was connected with one pole of a 1 to 4 cell battery (C); to the other pole was attached by flexible wire an ordinary steel pen or needle E. Required to draw a straight line between the arms of the compass as near

as possible to their intersection without making contact.[1] The distance between the arms at the point of contact (minus the thickness of the stylus) is, in general, a measure of deviation from the straight line at that point. The angle at which the arms of the instrument are set is not essential, since chords of every absolute length may be found, at some radius, in every angle; but the angle may be varied for various subsidiary purposes. A little calculation will determine for a given angle, the chords corresponding to each unit of length. Or since radius and chord are functions of each other the direct readings give at once the relative accuracy.

IV.

Preliminary Test of Precision.

In Writing Movement.

A number of conditions aside from the subjective condition of the person experimented upon, appear from prefatory trials to affect the accuracy of the result. (1) The position of the instrument relative to the body, (2) the muscles employed, (3) the support of the muscles employed, (4) the distance moved, affecting variously readjustments of the muscles while in motion, and (5) the rate of motion—all appear to affect the result. In order to make a beginning, even at the determination of these variables, it is necessary to prescribe conditions in at least four of the five. In the prefatory set of experiments with apparatus No. 1, the following conditions have been prescribed: The subject is seated before the table on which the apparatus rests; the forearm and hand are supported by the table; the instrument lies at an angle of about 50° to the right of a perpendicular to the frontal plane, now with the apex of the angle away from the subject and now with the apex of the angle towards him; the subject begins the writing movement 30 mm. from the zero point and moves continuously at any rate he pleases; the angle between the arms of instrument was in the experiments with adults 2°, in the experiment with school children $2\frac{1}{2}$°.

It is believed that the rates, instinctively chosen by the subject, may in the outset be taken in preference to any prescribed rate.[2]

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[1] The zero point is that point at which the stylus just makes contact with both arms of the compass.

[2] See Camerer's conclusion that the natural rate of movement is not a constant, but a gradually accelerated one. P. 138.

Method of Treating Results.

If a be the angle between the arms of the instrument, and l the distance of any point along the scale from the zero point, then half the distance between the arms of the instrument at any point is l sin. $\frac{1}{2}$ a. If t equal the number of trials at the point l, and h equal the number of hits, then $\frac{h\ 100}{t}$ is the empirically determined probability in per cent. of a deviation, $l\ sin.\ \frac{1}{2}\ a$.

If we assume $P_{l\ sin.\ 1\text{-}2\ a} = \frac{2}{\sqrt{\pi}} \int_0^h \varepsilon^{-t^2}\ dt$, the value of $h\ l\ sin.\ \frac{1}{2}\ a$ corresponding to the ascertained $P_{l\ sin.\ \frac{1}{2}\ a}$ can be obtained from the table of values of the Probability Integral

$$\frac{2}{\sqrt{\pi}} \int_0^t \varepsilon^{-t^2}\ dt.$$

for argument t. The value of l and of $sin.\ \frac{1}{2}\ a$ are known and h can be obtained from $h = \frac{t}{l\ sin.\ 1\text{-}2\ a}$.

The value of h, the measure of precision, may be determined in this way for as many different points along the scale as desired. In this work, the determination was made for every millimeter of the scale where there were trials and touches. For points at which no touches were made, of course, no percentage of touches to trials could be obtained.

The degree of precision was thus determined for each set into which the results obtained were classified.

It is not assumed by the foregoing calculation that the value of h is the same for different parts of the scale. The contrary is found to be the fact. This is brought out more clearly by taking the weighted mean of the values of h for each millimeter from 1 to 5, then of the values of h from 6 to 10, and so on in groups of 5 throughout the scale. By weighted means in this case it is meant that each value of h is multiplied by the number of trials made at that point; and that the sum of these products for the several points in the group is divided by the sum of the trials made at the several points in the group. These weighted means represent approximately the average precision of the class for that part of the scale. The values of h thus found are of course abstract numbers, and are significant only in comparison with each other. It seems desirable to know within what limits, in terms of ordinary standards of length, the deviations from a straight line fall. To find within what limits a certain per cent. of cases, say 68.3%, is likely to fall at the point for which the precision is h:

In general,

$$(h^1 x^1) : (h^1 x) : : x^1 : x$$

Where $(h^1 x^1)$=value of t from the Probability Integral table,

x^1=the known value of l sin. $\frac{1}{2}$ a

and $h^1 x$=.683.

$$\therefore \quad x = \frac{683.\ l\ \sin.\ 1\text{-}2\ a}{h^1\ l\ \sin.\ 1\text{-}2\ a}.$$

Since all the quantities on the right are known, the value of x is obtained by carrying out the indicated operations. This determination was made for the weighted mean of the values of h, for each 5 millimeters of the scale, in the case of each set of results obtained. The tables give in fractions of a millimeter twice the distances within which 68.3% of all deviations from the central line occur, and the weight or number of trials which entered into the determination of each.[1]

The object of this very laborious procedure was to ascertain and take account of the behavior of the pen point in every millimeter of its course and so to increase many fold the information to be obtained from each trial. For example, W. B. made 810 attempts to carry the pen point from 30 mm. to 0 mm. without touching. If the record of these attempts gave information only in respect to one point, namely, the point of touching, the sum of the weights of our information from the experiments could be only 810. But since by the foregoing method it is possible to take account of the average behavior of the pen point at nearly every millimeter of the

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[1] Details of the calculation:

Column I. Represented each mm. along the scale from 0-30 mm.

II. The number of touches recorded at each mm.

III. The number of trials made at each mm.

IV. The per cent. of touches to trials at each mm.

V. The per cent. of successes to trials at each mm.

VI. The value of ($h\ l\ sin.\ \frac{1}{2}\ a$) corresponding to the per cent. in V., obtained from the integral table.

VII. The values of ($h\ sin.\ \frac{1}{2}\ a$) obtained by dividing by the l from column I. Since ($sin.\ \frac{1}{2}\ a$) is constant these numbers are a measure of precision.

VIII. The products of the numbers in VII., by the corresponding numbers in III.,—i. e., precisions multiplied by their respective weights.

IX. The sums of the numbers in VIII., in groups of five.

X. The quotients of the numbers in IX., by the sums of the corresponding numbers from III. That is, x gives the weighted values of ($h\ sin.\ \frac{1}{2}\ a$) for each 5 mm. of the scale.

XI. From each of these, by the method described, page 83, the limits within which 68.3% of all deviations occur were determined. These numbers are given in the tables in connection with their respective weights obtained from column III.

scale, the numerical weight of information about the behavior of the pen in W. B.'s hand is 10,712.[1]

Eight (University) adults were tested in the manner described. Table XVIII. gives the number of trials made by each, the mean distances from the 0 point reached by each and the corresponding individual variations.

Table XIX. gives the results by the method explained page and shows accordingly the breadth of space in fractions of a millimeter within which 68.3% of all deviations fall. The numbers given are twice the deviation in one direction. The number before the colon is in each case the weight of the mean following the colon; thus 465 is the weight of the result .12 mm.

TABLE XVIII.

EXPERIMENTS ON PRECISION, WRITING MOVEMENT.

Persons	No. of Trials	Downward Movement		No. of Trials	Upward Movement	
		Av.	M. V.		Av.	M. V.
J. L.	50	9.6	2.7	50	11.1	3.6
J. A. B.	50	8.7	3.4	50	9.6	2.8
E. C. S.	50	8.2	3.1	50	6.	2.
F. B. D.	75	5.1	2 9	75	5.3	2.7
W. B.	100	5.	2.7	100	8.9	3.2
A. F.	50	4.7	2.1	50	5.3	1.9
T. L. B.	50	6.1	2.7	50	5.3	1.6
L. B.	75	3.9	1.9	75	4.2	1.6

Total number of trials, 1000.

[1] I again heartily acknowledge my indebtedness to Dr. Franz Boas for valuable advice, and at the same time must free him from all responsibility for the method which I have adopted.

TABLE XIX.

Part of Scale		I 1—5	II 6—10	III 11—15	IV 16—20	V 21—25	VI 26—30
		W't mm	W't mm	W't mm	W't mm	W't mm	W't mm
Subject L. B.	R Down	465 : .12	931 : .11	194 : .22			
	R Up	411 : .16	1057 : .12	443 : .16			
Subject T. L. B.	R Down	58 : .19	188 : .14	157 : .19			
	R Up	58 : .18	223 : .15				
Subject A. F.	R Down	96 : .12	217 : .14	50 : .17			
	R Up	60 : .16	201 : .15	144 : .19			
Subject F. B. D.	R Down	146 : .12	317 : .13	141 : .16	72 : .27	74 : .38	
	R Up	124 : .11	306 : .14	286 : .19		74 : .34	
Subject J. A. B.	R Down	24 : .22	156 : .19	192 : .22	56 : .29	59 : .41	
	R Up	6 : .39	141 : .22	143 : .21	159 : .26	116 : .31	
Subject E. C. S.	R Down	21 : .33	161 : .18	131 : .20	95 : .24	49 : .33	50 : .39
	R Up	65 : .16	222 : .14	150 : .26			
Subject J. L.	R Down		115 : .24	157 : .21	132 : .27		
	R Up		73 : .27	172 : .26	46 : .30	144 : .32	50 : .38
Subject W. B.	R Down	290 : .17	1470 : .19	1969 : .17	1626 : .19		
	R Up	244 : .16	1247 : .20	1872 : .20	1994 : .21		

1. Under the conditions named, the mean deviation varies in the eight individuals from about $\frac{.12}{2}$ mm to about $\frac{.38}{2}$ mm (×).

(×) That is, the deviations in one direction are one-half the numbers given in the table.

2. The mean variation of individual trials from the means ranges from about 30% to about 60% of the means.

3. It appears in general from Table XVIII. that the greater means go with the greater variations, i. e., the device shows the more accurate person, both by the relatively greater mean inaccuracy and by the greater irregularity in successive trials.

4. Owing to the fact that the maximum time required by any subject for the trials made at one time was never over five minutes, and to the fact that the writing movement is so thoroughly habituated, fatigue was practically excluded.

5. W. B. made about 800 trials, extending over a period of three weeks, without showing an observable improvement from practice.

School Tests on Precision.

Writing Movement.

With a few exceptions, the same pupils tested in the rate experiments were tested for precision.

Apparatus: Strips of platinum foil were pasted smoothly on plate glass so as to make an angle of 2½ degrees. A small steel needle, set in a common wooden penholder, served as stylus. Three to four Le Clanche cells constituted the battery. A telegraph sounder gave the signal when the needle touched the platinum.

Course of the Experiments: The child was seated in front of the table where the glass plate lay. The latter was placed in such a position that the line along which the stylus was to be drawn should make an angle of 45°—50° with the frontal plane of the subject, when the right hand was used; and an angle of 130°—135°, when the left hand was to be used. The stylus was in each case placed between the arms of the instrument, 30mm. from the 0 point, and drawn toward the 0 point until contact was made. Six trials were made with each hand, three movements in each case being made away from the body, and three toward the body. The point of contact was recorded. The child learned what he was expected to do as follows: (A.) In nearly every case he looked on while one or more of his comrades went through the tests. (B.) He received plain directions, e. g., "Take the pen; hold it so; put the point here; draw the pen so, without touching either side," etc. (C.) He was made to try several times, until it was quite certain that he knew what he was expected to do. In some cases with very small and very stupid children, it proved impossible to make them understand what they were expected to do. Such cases, after long and patient trial, were abandoned. But records were not excluded because of being unusually inaccurate, if it could be ascertained from the child's answers and efforts that he knew what to try to do. The entire series of precision tests in the schools of Worcester was taken with scrupulous attention to every detail by my wife, Mrs. Lotta Lowe Bryan.

Classification of Results.

The results were classified according to the age and sex of the subjects, according to the hand used, and according to the direction of movement. Each class of results was treated by the method described, page 180. The Table XX. gives the results. The numbers before the colons give the numerical weights of the results after the colons; the latter give twice the distance in mm. within which 68% of deviations fall. Results with a weight of 150 or more are printed in heavy type.

Table XX.

School Tests on Precision.

Part of Scale		I 1—5	II 6—10	III 11—15	IV 16—20	V 21—25	VI 26—30
	Age	W't mm	W't mm	W't mm	W't mm	W't mm	W't mm
Boys Right Down	6		17 : .69	152 : .45	304 : .44	229 : .43	
	7		62 : .40	237 : .38	424 : .41	302 : .39	
	8	15 : .23	168 : .30	399 : .30	490 : .33		
	9	49 : .16	283 : .26	508 : .30	610 : .31		
	10	45 : .25	251 : .26	472 : .29	214 : .35		
	11	84 : .14	305 : .24	504 : .26	108 : .25		
	12	61 : .17	247 : .26	423 : .28	285 : .27		
	13	153 : .17	401 : .19	294 : .21	203 : .28		
	14	173 : .14	433 : .20	571 : .23	238 : .27		
	15	71 : .17	293 : .24	455 : .25	96 : .29		
	16	48 : .27	256 : .23	366 : .25	78 : .28		
Boys Right Up	6		25 : .54	155 : .44	326 : .43	154 : .38	
	7		72 : .45	263 : .38	448 : .40	203 : .36	
	8	10 : .26	163 : .33	370 : .34	196 : .32		
	9	39 : .16	246 : .29	524 : .31	374 : .28		
	10	22 : .23	208 : .31	466 : .31	214 : .29		
	11	45 : .27	288 : .27	504 : .27	108 : .30		
	12	70 : .17	302 : .24	366 : .26			
	13	80 : .16	333 : .25	392 : .24			
	14	118 : .16	421 : .22	571 : .22	120 : .25		
	15	120 : .19	398 : .20	191 : .22			
	16	97 : .19	323 : .22	231 : .21			
Boys Left Down	6			62 : .59	171 : .54	305 : .54	78 : .53
	7			52 : .50	218 : .57	411 : .52	297 : .52
	8		31 : .52	165 : .41	358 : .48	476 : .41	99 : .42
	9		59 : .36	215 : .45	484 : .45	609 : .41	
	10		73 : .42	248 : .38	462 : .42	320 : .38	
	11	4 : .66	83 : .35	253 : .36	463 : .44		
	12	9 : .20	114 : .33	320 : .37	462 : .34		
	13	33 : .32	200 : .24	360 : .39	381 : .35		
	14	13 : .20	184 : .30	418 : .33	556 : .34	239 : .34	
	15	47 : .19	263 : .25	412 : .27	282 : .31	96 : .34	
	16	31 : .30	194 : .24	310 : .24	228 : .33		
Boys Left Up	6		10 : .63	57 : .48	166 : .56	289 : .56	231 : .53
	7		43 : .36	128 : .40	283 : .49	427 : .48	299 : .51
	8		42 : .40	181 : .40	342 : .47	453 : .45	201 : .51
	9	8 : .26	104 : .32	287 : .37	498 : .45	615 : .41	129 : .44
	10	26 : .20	135 : .26	295 : .35	464 : .39	318 : .41	
	11	11 : .20	93 : .37	298 : .34	473 : .40	214 : .36	
	12	38 : .16	122 : .29	328 : .35	458 : .36	96 : .35	
	13	58 : .17	223 : .22	377 : .29	471 : .34	302 : .37	
	14	94 : .16	263 : .22	460 : .32	572 : .32	120 : .33	
	15	56 : .17	242 : .26	406 : .27	575 : .30		
	16	59 : .19	219 : .22	348 : .28	231 : .29		

TABLE XX.—*Continued.*

SCHOOL TESTS ON PRECISION.

Part of Scale		I 1—5		II 6—10		III 11—15		IV 16—20		V 21—25		VI 26—30	
	Age	W't	mm	W't	mm	W't	mm	W't	mm	W't	mm	W't	mm
Girls Right Down	6	3	.48	56	.29	212	.43	387	.40				
	7			41	.53	224	.42	399	.40	274	.39		
	8	21	.24	182	.23	352	.32	359	.33				
	9	24	.21	171	.27	394	.32	506	.31				
	10	35	.22	243	.24	449	.31						
	11	89	.18	344	.24	487	.27						
	12	74	.16	235	.22	422	.30	500	.30				
	13	53	.21	265	.26	463	.26	102	.28				
	14	92	.13	304	.22	355	.24	378	.30				
	15	56	.21	294	.22	330	.27	366	.30				
	16	15	.31	142	.26	188	.28						
Girls Right Up	6			79	.34	218	.43	307	.43	83	.41	84	.43
	7			82	.36	265	.39	427	.37	185	.35		
	8	17	.31	169	.29	363	.29	361	.32				
	9	26	.30	191	.27	411	.34	313	.32				
	10	38	.27	294	.34	476	.25	203	.28				
	11	48	.24	306	.27	506	.25						
	12	55	.25	283	.25	453	.27	303	.27				
	13	80	.18	341	.23	390	.24						
	14	86	.20	354	.22	460	.22	96	.29				
	15	75	.22	321	.23	264	.23	93	.29				
	16	69	.15	213	.22								
Girls Left Down	6			4	.50	82	.48	202	.50	356	.57	84	.45
	7			8	.35	122	.45	287	.49	434	.48	93	.42
	8			25	.34	147	.48	365	.48	275	.40		
	9			33	.43	172	.43	383	.51	318	.42		
	10			42	.36	176	.44	413	.47	203	.38		
	11			79	.31	276	.41	465	.38	465	.38	105	.40
	12	3	.21	72	.34	253	.40	440	.43	203	.34		
	13	9	.74	146	.30	351	.35	482	.34				
	14	7	.57	169	.31	348	.31	447	.32	191	.41		
	15	28	.22	176	.24	323	.32	427	.34	184	.39		
	16	6	.48	122	.27	212	.32	150	.33				
Girls Left Up	6			23	.40	83	.44	202	.54	370	.55	84	.48
	7			7	.49	86	.40	265	.53	343	.47		
	8	9	.34	62	.27	208	.42	348	.42	447	.44		
	9			56	.40	208	.39	401	.42	505	.45	108	.41
	10	17	.27	102	.28	239	.35	421	.42	298	.42		
	11	57	.13	178	.23	338	.32	451	.37	410	.39		
	12	34	.22	148	.26	347	.32	465	.34	303	.39		
	13	23	.12	136	.28	335	.34	470	.36	203	.39		
	14	17	.21	146	.32	316	.33	342	.35	281	.38		
	15	24	.18	169	.27	348	.32	345	.32	366	.37		
	16	21	.27	124	.25	222	.27	99	.32	31	.33		

Trustworthiness of Results.

1. Owing to the fact that the arithmetical mean of the individual results was not determined, it is not possible to give in the ordinary way the mean individual variation. In order to exhibit, however, the relative individual variation, I have determined for each of the 88 sets of results, the smallest distance along the scale within which two thirds of the individual results fall. In the following table these values, each divided by 2, are given as approximate measures of the individual distribution. It will be observed that the individual variation is greater for the left hand than for the right, and tends to decrease with advancing age.

TABLE XXI.

Table of values of smallest distance along the scale within which two thirds of hits fall, in mm.

2

	6	7	8	9	10	11	12	13	14	15	16
B. R. D.	4.	4.5	3.5	3.9	3.7	3.6	4.1	2.9	3.3	3.2	2.9
B. R. U.	4.5	4.	3.8	3.5	3.	3.1	3.2	3.	3.2	2.6	2.8
B. L. D.	5.4	4.5	4.2	4.3	4.3	4.4	3.8	4.9	4.	4.	4.1
B. L. U.	5.4	5.4	5.3	5.2	5.2	4.5	4.5	5.2	5.2	4.	3.9
G. R. D.	3.9	3.5	4.5	4.	3.8	3.5	4.6	3.5	3.6	3.6	3.
G. R. U.	4.	3.9	3.8	4.2	2.7	3.	3.6	3.	2.7	3.1	2.7
G. L. D.	4.6	4.8	3.5	3.8	3.9	4.	3.9	3.9	4.	5.2	3.4
G. L. U.	5.1	4.2	4.	4.2	5.5	6.3	5.	4.6	4.5	4.	3.5

2. A method of determining within certain limits the probable trustworthiness of results is afforded by a comparison of results from boys and from girls.

It appears that the maximum difference between a result for boys and the corresponding result for girls is .09 mm.; that the mean difference between boys and girls for the right hand is .004 mm., and for the left hand .007 mm.; and that 68.3% of the differences do not differ from the mean, for the right hand more than .02 mm., and for the left hand more than .029 mm.

This calculation is analogous to that for determining the mean variable error after the constant error has been found.

These numbers then, .02 mm. for the right hand and .03 mm. for the left hand, are measures of the probable mean variation of the results given in the table.

These numbers are unfortunately not so insignificant as may at first appear. The total reductions in the mean deviation between 6 and 16 are for the right hand about .23 mm. and for the left hand about .32 mm. This would show the average yearly reduction of the mean deviation to be for the right hand about .023 mm., and for the left hand about .032 mm. An examination of the table of actual yearly reductions of the mean deviations shows that the yearly plus or minus change in the mean deviation ranges from 0 to .098 mm. for the right hand, and from 0 to .085 mm. for the left hand. It appears, therefore, that in many cases the limit of doubt attaching to the results is greater than the change by growth in one year, and in some cases considerably more.

Although three times as many experiments were made upon an individual in each set of precision tests as in the rate tests, and although by the method of treatment each single trial by the pupil furnished information as to the precision at many points in its course, so that the numerical weights of the precision results are many times greater than the numerical weights of the rate results,—yet the former are still insufficient to define clearly the amounts of yearly growth. This result, which evidently comes from the much greater variability in the power to make precise movements than in the power to move at a maximum rate, is itself significant; but it limits greatly the possibility of deriving trustworthy conclusions concerning the development of precision of movement. In the following, only those conclusions will be given which stand apparently clear of doubt, in connection with all information possessed by the author for determining the several degrees of probability.

Variation in the Precision at Different Parts of the Scale.

It may be observed in almost every set of results that the mean precision increases as the apex of the angle is approached. This does not, of course, mean that fewer touches per hundred trials are made, but fewer, in proportion to the space between the arms of the instrument. It seems probable that this is due to the more perfect concentration of attention as the task becomes more difficult. In all comparisons made in the following treatment, as, for example, between right and left, or as between one age and another, the results obtained within the same 5 mm. of the scale are compared.

Extreme Limit of Variation.

The total reductions in the mean deviations between ages 6 and 16 are as follows: Boys, right, down, .26 mm.; boys,

right, up, .24 mm.; girls, right, down, .21 mm.; girls, right, up, .21 mm.; boys, left, down, .33 mm.; boys, left, up, .34 mm.; girls, left, down, .33 mm.; girls, left, up, .28 mm. In each case the results are given in terms of 2x, where x is the mean deviation in one direction from the straight line which bisects the angle *a*.

Yearly Variation.

To determine the yearly gain or loss in precision as measured by the mean deviations (Table XX., pp. 186, 187): Subtract each result with a weight of 150 or more from the result just above it in the table; e. g., in table, B. R. D., subtract each result for age seven from the result for age six which falls in the same group (.38 from .45, .41 from .44, and .39 from .43). Take the mean of these differences. Proceed in like manner with the results for ages 7 and 8, 8 and 9, etc. This calculation was made for all the results in Table XX. No conclusion is drawn from these results which is not justified by the several individual results.

The most obvious fact which appears is the great gain made between ages 6 and 8. This can be shown by placing side by side the gain in those years and the total gain from 6 to 16.

TABLE XXII.

	Boys.				Girls.			
	R. Down	R. Up	L. Down	L. Up	R. Down	R. Up	L. Down	L. Up
	mm.							
Reduction of Mean Deviation between 6–8.	.128	.096	.098	.096	.090	.125	.095	.115
Reduction of Mean Deviation between 6–16.	.26	.24	.33	.34	.21	.21	.33	.28

Turning to the records from 12 to 16, I am unable to draw any conclusion as to the effect of the physiological changes in that period upon the degree of precision, except that the effect is too small to appear clearly from the amount of data possessed.

R. VERSUS L.

In the case of 305 individuals (boys and girls of 6, 9, 12, 15 and 16), each individual record was examined with reference to the superiority of the right hand over the left. Upward movements with the right hand were com-

pared with upward movements with the left hand, and likewise downward with downward. The six right hand records for each individual were paired with the six left hand records by taking the numbers in the order in which they stand in the original records; i. e., the pairing was determined by chance. The following table shows the result of this comparison:

TABLE XXIII.

Age	No. of Comparisons.	R. Superior +	Equal. 0	R. Inferior −	% +	No. of Persons in whom R. was Always Best.
6 Boys,	153	123	3	30	78.8	9
Girls,	168	130	4	34	77.4	7
9 Boys,	258	215	6	37	83.3	20
Girls,	216	168	9	39	77.7	5
12 Boys,	192	154	6	32	80.2	9
Girls,	204	167	4	33	81.3	8
15 Boys,	192	135	15	42	70.3	1
Girls,	186	126	18	42	67.7	7
16 Boys,	156	101	8	47	64.7	3
Girls,	102	72	6	24	70.6	3
Total,	1830	1391	79	360	76.	72 = (23 + %)
Persons,	305					

The table shows (A) that only in the case of 72 individuals out of 305 (23 %) does every right hand trial exceed in precision the left hand trial with which it is compared. (B) That in 1,391 out of 1,830 comparisons (76%) the right hand result is superior to the left. (C) That the per cent. of advantage by the right hand is less at 15 and 16 than at 6, 9, or 12 years of age.

A comparison of the mean values given in the right hand tables, pages 88 +, with those from the left hand tables shows in 78 cases out of 83 the right hand superior to the left.

The amount of superiority of right over left varies remarkably with age. If the values of (r–l) be determined by sub-

tracting each r value in the table, with a weight of 150 or more, from the corresponding l value, and if the mean of these (l–r) values be determined for each age, it appears that for both directions of movement, for boys and for girls, there is a decrease in the value of (l–r) with age. If the mean difference between r and l for each age be determined (including in the mean the results for both directions of movement, and for both sexes), we have:

Average Superiority of R. over L. in terms of x. mm.

Age.	6	7	8	9	10	11	12	13	14	15	16
Av. x,	.122,	.12,	.114,	.101,	.097,	.09,	.087,	.06,	.07,	.038,	.025

Boys and Girls.

An examination of results on page 188 shows that with either hand, the boys are very slightly superior to the girls in precision.

Mean superiority of boys' right hand, .004 mm.
" " " " left " .007 mm.

In 47 comparisons of right hand results the boys are superior 24 times; girls 13 times; boys and girls equal 10 times.

In 52 comparisons of left hand results boys are superior 27 times; girls superior 22 times; boys and girls equal 3 times.

In the 99 comparisons, boys were superior 51.5%; girls superior 35.3%; boys and girls equal 13.2%.

Precision Experiments.

Probing Movement.

Gowers (13) quotes Blix as proposing to determine the degree of incipient ataxia by requiring the patient to tap several times with a pencil, endeavoring each time to strike a fixed point on the paper. The distances of the points actually struck from the fixed point furnish material for estimating the degree of ataxia when compared with results from normal individuals.

The objection to this method on account of the great labor involved (Gowers I. 5), may be removed by using the following device: Paste smoothly upon a slab of plate glass a one □ cm. piece of platinum foil perforated by a circular hole 1 mm. in radius and connected with one pole of a small battery. To the other pole of the battery attach by a fine flexible wire a steel needle, set in a wooden pen-holder. Required to hold the point of the needle at a fixed distance perpendicularly above the centre of the hole, and at command to tap the glass

within the hole. A telegraph sounder gives the signal if contact is made. If the hole in the platinum be of such a size that a considerable per cent. of trials falls within, and another considerable per cent. falls without it, then the number of hits within, divided by the whole number of trials is the empirical probability of hitting within that area. The radius x of the hole being known, the value of h can be determined from the Probability Integral. If desired, an x can be determined corresponding to a probability .683, that is, one can determine from the ascertained probability, and the known value of x, the x which must be used in order that 68.3% of the trials shall be successful.

If an electric counter were used (such as that of Ewald or the apparatus of Dr. E. C. Sanford, already mentioned), and if one hundred trials were made, the reading on the clock face would be the empirical probability (p) in per cent. of failing to hit within the hole; (100-p) would then equal the probability of succeeding. If the size of the hole were kept constant, the values of t in the Table of the Probability Integral $\frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2}\,dt$ would always be equal to $k\,h$, the value of k varying with the unit in which the radius of the hole is expressed. It would be very easy to have the Probability Integral Table printed upon a convenient card-board for immediate reference in the clinic; so that if the average value and variation of h in normal cases were determined, it would be a matter of a few moments to obtain this test of incipient ataxia.

As a clinical test, however, this method is open to one serious danger. Probably for the reason that the movement is an unusual one, the subject makes very awkward movements at first; and owing to the fact that the movement is not difficult to learn, he makes very rapid improvement. This is shown in the following record made by myself. Each number in the table represents the number of failures to hit within the hole, ten trials being made in each case. In one set of trials the wrist moved and the other joints were kept still; in a second set of trials the elbow alone was moved; and in a third set the shoulder alone was moved. The conditions were kept as nearly possible the same in successive trials. It is evident in each case that the comparatively small amount of practice has greatly reduced the number of failures.

TABLE XXIV.

Table showing number of failures in ten trials; 60 sets of ten trials each. Distance moved 1 cm. Trial every 2 seconds.

	RIGHT.			LEFT.		
No. trials.	Shoulder.	Elbow.	Wrist.	Shoulder.	Elbow.	Wrist.
60	4	8	2	3	3	6
60	5	2	1	3	4	7
60	4	2	3	4	6	4
60	2	1	0	9	5	4
60	1	2	0	5	4	0
60	2	4	1	4	5	0
60	1	2	0	6	2	1
60	0	0	0	7	1	0
60	1	1	0	3	0	1
60	0	3	0	1	3	4
600	20	25	7	45	33	27

SCHOOL TESTS ON PRECISION.

Probing Movement.

With few exceptions the children tested by the foregoing methods were tested also for precision in the movement just described. Five trials were made with the right hand and five with the left. A board was held in position 6 mm. above the apparatus; the pen-holder was each time lifted until its upper end touched this board. It cannot be guaranteed that the pen will always be held in a perfect perpendicular, and accordingly the minimum distance to be moved, 6 mm., was sometimes slightly increased. If we assume what is quite certain, that in no case the pen was permitted to slant so much as thirty degrees, the maximum distance moved was always less than 8 mm.

The forearm was allowed to rest upon the table. The pen was directed mainly by the movement of the wrist, in a slight degree sometimes by movement of the elbow. Concrete directions by word and example, as in the writing move-

ment test, were given. No record was taken unless and until the child gave satisfactory evidence that the task required was understood. The results were classified according to the age and sex of the pupil and according to the hand used.

In the foregoing pages (192-193) the method of treating such results has been shown. It is only necessary here to say that in the manner described the probability of tapping within the hole without touching the platinum was determined for each set into which the results were classified. From this probability and the known value of x, the radius of the hole, were determined the values of h in each case, and the radii of circles within which in the several cases two thirds of the results would probably fall. Table XXV. gives these results and Chart V. shows graphically the same facts.

TABLE XXV.

Table showing in mm. the radii of circles within which 68.3% cases would fall.

AGE.		6	7	8	9	10	11	12	13	14	15	16
Boys,	Right	1.10	.97	.87	.80	.67	.65	.53	.58	.60	.42	.44
	Left	2.09	1.69	1.50	1.05	1.01	.90	.86	.87	.96	.79	.94
Girls,	Right	.91	1.01	.85	.86	.69	.69	.59	.61	.53	.49	.40
	Left	1.84	1.24	1.15	1.27	1.05	1.02	.83	.97	.74	.82	.77

EXTREME LIMITS OF VARIATIONS.

The amounts of decrease in the mean deviations between ages 6 and 16 are shown by the foregoing table to be: for boys' right .68 mm.; boys' left 1.30 mm.; girls' right .61 mm.; girls' left 1.10 mm.

These numbers are all larger than those in the corresponding table for the writing movement, page . That is, the mean deviations are very much larger and the decrease in the absolute size of the mean deviation between ages 6 and 16 is very much greater. The relatively great gain between ages 6 and 8 does not appear here so decisively as in the writing movement.

As in the writing movement, the left hand reduces the mean deviation much more than does the right hand.

Except at the age of six, where boys are inferior to girls, no decisive difference appears between the sexes.

Résumé of Results from Precision Experiments.

1. In normal individuals, the precision of voluntary movement is subject to much greater variation than is the maximum rate of movement. This test will probably distinguish pathological from normal deviations surely, only when the ataxia exists in a considerable degree, or when many tests are made. (P. 189.)

2. The absolute size of deviations from the movement attempted decreases much more rapidly in the two or three years following the age of six than later. This is particularly true in the case of the right hand. (P. 190.)

3. In right-handed persons, the right hand is superior to the left in precision, in about 80% of individual cases. Between ages 6 and 16, the deviations of left hand movements decrease by a greater absolute amount than do those of right hand movements. This is true for boys and for girls, in both directions of the familiar writing movement, and in the unfamiliar probing movement. (P. 191.)

4. The errors are, of course, greater with the unfamiliar probing movement under the conditions described than with the familiar writing movement. The decrease of the absolute size of the mean deviations is also greater. (P. 195.)

5. There is little mean difference in precision between boys and girls. These results indicate a slight superiority in favor of boys. (P. 192.)

Strength and Endurance.

Out of a large number of strength and endurance tests, only those will be reported at present which bear upon the question of bilateral development.([22])

Apparatus: The literature of dynamometry shows general dissatisfaction with the apparatus and methods which have been employed.[1]

It has been shown that varying mechanical factors, in the instrument, in the mode of gripping, or in the size and shape of the hand, co-operate with the quantity of force exerted to determine the record and therefore render the record doubtful. The following comparisons are made with the assumption that in the long run neither hand of the same individual would likely have any mechanical advantage over the other. A form of apparatus was devised essentially similar to that proposed by Hamilton([20]), (that is, a mercury dynamometer, the mercury balanced by water, pressure being applied to a rubber bulb and transmitted by water to the mercury). Two

[1]For a partial Bibliography of Dynamometry see Reference Handbook of Medical Sciences, II. 544. Cf. also Vierordt([21]).

of these dynamometers were made and placed in the same frame about 40 cm. apart so that pressure could be applied to both bulbs at once, or to either at pleasure. In the following experiments, ample time was given to rest between trials, except that when the two hands were used successively, the succession was immediate. The order in which the hands were used was alternated in successive tests. The following table gives the number of times the preferred hand was superior in strength in the cases of seven (University) adults:

TABLE XXVI.

Subjects		J. A. B.	T. L. B.	G. S. H.	W. B.	F. B. D.	J. L.	E. C. S.	Total
No. of Trials		13	18	15	39	18	8	13	124
Preferred hand stronger	Hands used separately	5(6)	7(9)	0(2)	17(19)	7(9)	4(4)	6(7)	46
	Hands used together	5(7)	6(9)	11(13)	18(20)	5(9)	4(4)	5(6)	54
Total		10	13	11	35	12	8	11	100

The numbers in parentheses show the number of trials.
Number of cases, preferred hand stronger=100=80.6%.
" " " " " not as strong=18=14.5%.
" " " hands equal=6=5.%.

It is shown that the preferred hand exerts more strength than the other hand, whether the hands are used separately or together in about 80% of the cases. This is almost precisely the probability obtained from the school tests, that a right side joint will be faster in a given case than the corresponding left side joint. In this connection the claim of Féré([23]), that strength and rate vary together, and the suggestion that rate depends in part upon the intensity of innervation, will be recalled. Compare (p. 191) the probability of (R > L) in precision. Cf. Binet([30]).

The same experiments tabulated above were used also to determine whether more strength can be exerted by a hand working alone, or by the same hand when the other hand is working also. The results show that 5 of the 6 adult subjects were able to exert more strength with the hand working alone in most, but not in all cases.

In 60% of (112) cases, the hand working alone was stronger than when the other hand was working also.

In 27.5% of cases, the hand working alone was not so strong as when the other hand was working also.

In 12.5% of cases, the result was the same.

Table XXVII. shows the number of trials made by each subject, the mean height in cm., to which the column of mercury could be raised by each hand working alone, and by each hand working at the same time with the other.

TABLE XXVII.

MAXIMAL GRIP.

		Hands Separately		Hands Together	
		Right	Left	Right	Left
	No. of Trials	cm.	cm.	cm.	cm.
T. L. B.	(9)	115	112	108	106
E. C. S.	(8)	79	75	74	72
J. L.	(4)	80	66	76	61
F. B. D.	(9)	113	109	109	106
W. B.	(20)	99.6	96.6	99	95.6
J. A. B.	(8)	84.4	78	84.2	82

NOTE ON ENDURANCE TESTS.

A few tests were made to determine the number of seconds an amount of force equal to about two thirds of the maximum force could be exerted. It came out very clearly that the right hand has greater endurance than the left; that the endurance of each hand is greater when working alone than when working at the same time with the other; and that the endurance of each hand is lessened if the other hand has been wearied by an endurance test. These results, however, were obtained only upon a single subject, and require verification.

TABLE XXVIII.

SUBJECT W. B. TABLE SHOWING RESULTS OF ENDURANCE TESTS.

		Hands Working Separately		Both Hands	
	No. of Trials	Right Hand	Left Hand	Right	Left
First Set	8	36″	30″	26″	26″
Second Set	10	36.1″	30.5″		

Av. endurance of right hand when left was wearied 32.6″
" " " " " " " " unwearied 33.7″
" " " left " " right " wearied 28.4″
" " " " " " " " unwearied 31.2″

Average superiority of right over left when right hand came before left: 10.″

Average superiority of right over left when right hand came after left: 1.2″

THEORETICAL.

In the foregoing pages I have given the immediate results of the experiments made, without theoretical comment or inference. Following are suggestions of probable conclusions from the facts:

I.

The maximum rate of voluntary rhythmically repeated movement is probably a critical test of voluntary control.

(a) While it does not appear that the rate of rhythmical "resulting movements" is identical with the rate of innervation, it can scarcely be doubted that the rate and intensity of innervation affect the rate of resulting movements. Von Kries says:

"Das wir in dem Rhythmus der Muskel Anschwellungen wirklich den Rhythmus der Inervation erhalten, das wird, wie ich glaube, kaum bezweifelt werden." This has been assumed by most studies of the rate of innervation. (24, p. 9. See also 25, p. 10.)

If this be true the decrease in the rate of the muscle rhythm (Griffiths, see p. 138) with fatigue is in all likelihood cause or part of the cause of the simultaneous decrease in the rate of the "resulting movements."

(b) The maximum rate of rhythmically repeated voluntary movement is probably a test of the power of voluntary arrest and reversal.

It was shown from experiments by Von Kries (p. 138), and confirmed (p. 150), that variations in the amplitude of movement within wide limits do not affect the rate. This paradoxical result,—which I have found to hold also for eye movements within the angle of usual movement,—is due to one or both of the following causes. For small distances, and especially for distances less than those usually passed over, the times of arrest, reversal, and of passing through the space nearest the points of reversal, may be slightly increased. In all cases the sum of these times must be so great in proportion to the time occupied in passing through the middle space that a slight increase of the latter is inappreciable.

That is to say, the rate of tapping is almost identical with the rate of voluntary arrest and reversal.

(c) The maximum rate of movement probably furnishes a test of the general condition of the central nervous system.

In this connection, Dresslar's([26]) demonstration that mental excitement increases the rate, and that each day the rate probably varies with the tone of the central nervous system, is especially significant.

(d) The maximum rate of movement probably furnishes a test of the condition of the nerve centers by which the muscles involved in the movement are controlled.

If the conclusion of Mosso([27]), Maggiora([28]) and Lombard ([29]) be correct, that the working of a joint produces central fatigue, it is probable that the cases in which working a joint was followed by a lowering of its rate are to be explained in part, at least, as due to the effect of central fatigue. The fact that the rate of a joint is lowered by local fatigue while the rate of other joints remains unaffected, indicates no finer physiological differentiation in the central nervous system than the fact that we are able voluntarily to move one joint while the adjacent joints remain still. It would strongly confirm this view if it should appear that fatigue through one joint affects the rate of the corresponding joint on the other side.

II.

The History of the period from 12 to 16 in girls (see pages 161 and 174) and from 13 to 16 in boys, exhibiting in turn acceleration, decline and recovery of rate ability, presents what is, at any rate, a suggestive analogy to the course of ordinary over-tension, fatigue and recovery of the nerve centers. It would seem something more than a reasonable surmise that the general acceleration of the rate in girls from 12 to 13, and in boys from 13 to 14, is an expression of high tension in the nerve centers in many individuals at those ages; that the decline following is an expression of nervous fatigue consequent upon the functional changes at those periods; and that the re-acceleration is a sign of recovery from that fatigue. It is significant that (page 174) the antecedent acceleration and the decline are more extreme in girls than in boys, and that the girls recover more slowly. It seems not unlikely that these facts may prove of hygienic significance.[1]

[1] Note the result of Bowditch (8, 10 and 22, Reports State Board of Health of Mass.), of Peckham (6th Report State Board of Health of Wisconsin), and of others reported by them showing that boys exceed girls in weight and height at all ages from 5 to 18, except from about 12½ to 14½. Dr. Gerald M. West tells me that the measurements of Worcester school children by Dr. Franz Boas and himself show the same result. See also Prof. Bowditch's explanation by the theory of antagonism between growth and reproduction. (Op. cit. 8, 283.)

III.

The fact that the hand is at first inferior in rate ability to the arm (pp. 169-172) is perhaps explained, and its genetic significance is emphasized, by the observation made upon nearly, if not quite, every child of five and six, that the clasping tendency is still very strong. This is shown by the evident ′—— – | ′—— – | ′—— – | rhythm of up and down strokes or by testing the force of downward pressure.

IV.—Résumé of Results Touching Bilaterality.

I. In the right-handed subjects, the right hand and arm are superior to the left in strength, rate and precision in a majority of trials. In few subjects is the right hand superior in every trial. The probability from these researches that in a single trial taken at random the right hand will exceed the left in strength, rate or precision is about 75% to 80%, the probability that the right hand will not be inferior to the left in such a trial is about 85% to 90%.

2. The effects of effort through either upper extremity are probably shared in some degree by both. (a) Between the ages 6 and 16 the right hand and arm very little, if at all, outgrow the left in rate ability. Since it is certain that the right side joints have vastly more use, either the growth of rate ability has not been determined by use, or the effects of use on the right side have been shared by the corresponding joints on the left side. The fact that Dresslar found no effect from practice is to be taken in connection with the fact that his records show very little effect from practice upon the right hand. The preliminary practice, while perfecting apparatus, had, he thought, trained his hand about to its maximum before records were taken.

(b) Between the ages 6 and 16 the mean deviation from the movement intended is reduced by a greater absolute amount by the left hand than by the right. We are not justified in assuming that reduction in the absolute size of the mean deviation by a certain amount means the same degree of gain in voluntary control, whatever the mean deviation from which the reduction is made. It is certainly easier to reduce a mean deviation x to $\frac{x}{2}$ than to reduce the latter to zero. It would be a rash conclusion that there is a gain in precision by the right hand between 6 and 8 equal to its gain between 8 and 16, because the reduction in the absolute size of the mean deviation in the first period is about equal to that in the second. The fact that between the ages 6 and 16 the

reduction in the absolute size of the mean deviation, in the case of the unfamiliar probing movement, is about six times as great as that made in the case of a movement constantly practiced, can certainly not mean that there is actually a greater gain where there is infinitely less use.

The fact that decrease in the size of the mean deviations from the movement intended is greater with the left hand than with the right, with the right when it is little developed, in the less practiced movement, and that as the deviations become less, their reduction becomes slower,—must rather be held to indicate that great absolute reductions are characteristic of relatively low development; and that slow and steady reductions are characteristic of comparative escape from physiological ataxia. It is, nevertheless, certain that the right hand does not outgrow the left; and the fact that, at 15 and 16 years of age, the probability of $(R > L)$ is less than at 12, 9 or 6 years of age, seems to indicate that the left has actually gained upon the right. At all events the fact that the left hand should make such relative improvement both in ability to carry out an unpracticed (probing) movement, and in ability to carry out a movement in which the right hand has had all the practice, tends to confirm the probability of bilateral effects of practice. It is, of course, not to be forgotten that the practice in this case is largely mental. (See Stumpf([31]), Cattell & Fullerton([32]), Camerer([33]), Fechner ([34]).

(c) The amount of force which can be exerted through one hand and the time during which it can be exerted depend upon whether at the same time or just preceding, force has been exerted through the other hand.

(d) The maximum rate of a joint is *possibly* affected by the exertion of the corresponding joint on the other side in the time just preceding.

4. Corresponding joints have generally the same periods of acceleration and retardation of growth. But there is nearly always a considerable bilateral asymmetry of development; and the asymmetry is generally the greater as the growth is more rapid. The fact that the boys' right arm grows faster in rate-ability than that of girls, and grows more asymmetrically as compared with the left, that the hands outgrow the arms and show more bilateral asymmetry, and that periods of rapid growth are generally periods of increasing bilateral asymmetry, show that bilateral asymmetry is not to be regarded as abnormal, but rather as in some degree an attendant and sign of growth.

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NOTE.—I heartily acknowledge my indebtedness to all the members of the Psychological Faculty of Clark University for advice and assistance in this and other work in the University. Above all I am under obligation to Pres. G. Stanley Hall for directing me to and in the following research.

Specific credits are given in the text.

EXPLANATION OF RATE CHART I.

1. Abscissa=time. Years noted at bottom of chart.
2. Ordinate=rate. Seconds indicated on margin.
   The base line is assumed to represent 4 taps per second.
   All points are then fixed with reference to that line.
3. The solid lines connect the mean-rate points of successive years.
4. The inside dotted lines represent the limits of mean variation of the means.
5. The outside dotted lines represent the limits of mean individual variation.
6. Boys' charts on the left of the middle line; girls' on the right.

EXPLANATION OF (R-L) CHART II.

1. Abscissa=time. Years noted at bottom.
2. Ordinate=values of (R-L). Base line: (R-L)=0. R-L=1 indicated on margin.

———————— = Finger.
. . . . . = Wrist.
–o–o–o–o–o– = Elbow.
++++++++++++++++++ = Shoulder.

Boys to the left; girls to the right of middle line.

EXPLANATION OF BILATERAL ASYMMETRY CHART III.

Abscissa=time. Years noted at bottom of chart.

Ordinate=values of $\sqrt{\mu_R^2+\mu_L^2}$=function of mean individual variations of R and of L, and of μ_B=mean variation of individual (r-l) values from this mean. Value of ordinate=1 tap per sec. indicated on margin.

———————— = Line connection values of $\sqrt{\mu_R^2+\mu_L^2}$.
. . . . . = " " " " μ_B.
Base line = " whose ordinate is zero.

Boys to the left; girls to the right of middle line.

EXPLANATION OF PRECISION: PROBING MOVEMENT CHART IV.

1. Abscissa=time. Years noted at bottom of chart.
2. Ordinate=values of the radii of circles within which 68.3% trials would fall. Values of ordinates, 1 mm. and 2 mm. indicated on margin.

The upper lines represent the records for the left hand.
The lower lines " " " " " right hand.
Boys to the left; girls to the right of middle line.

Chart I. Rate.

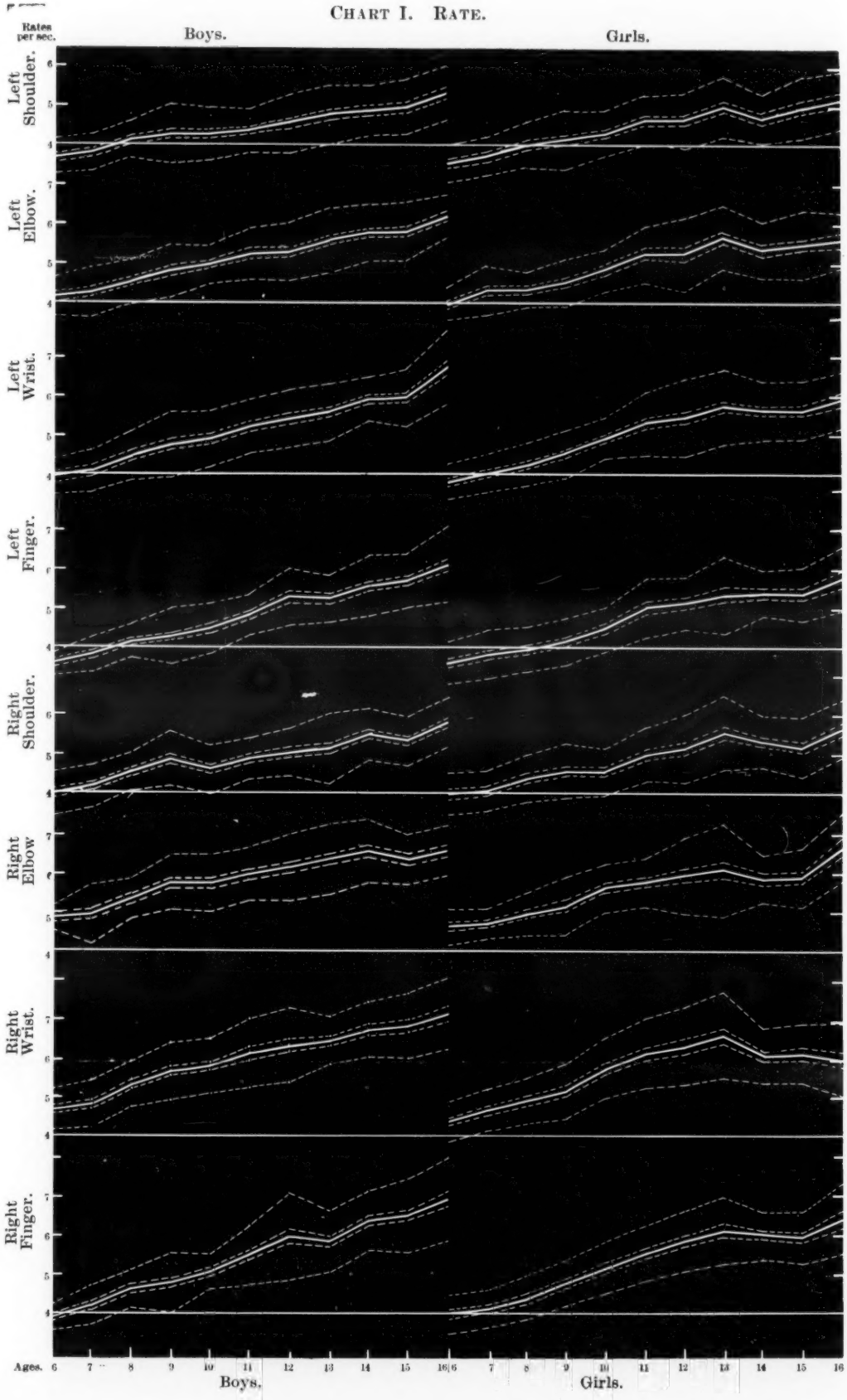

Rates per sec.
Boys.
Girls.
Left Shoulder.
Left Elbow.
Left Wrist.
Left Finger.
Right Shoulder.
Right Elbow
Right Wrist.
Right Finger.
Ages. 6 7 8 9 10 11 12 13 14 15 16
Boys.
Girls.

CHART II. RATE: (R.–L.).

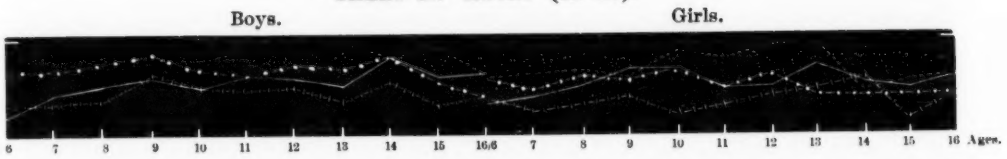


CHART IV. PRECISION: PROBING MOVEMENT.

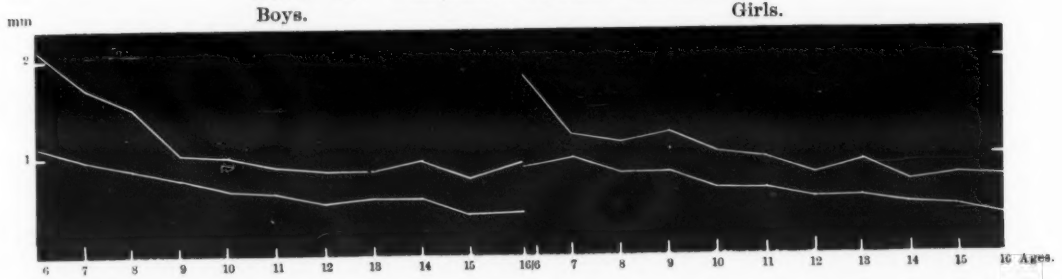
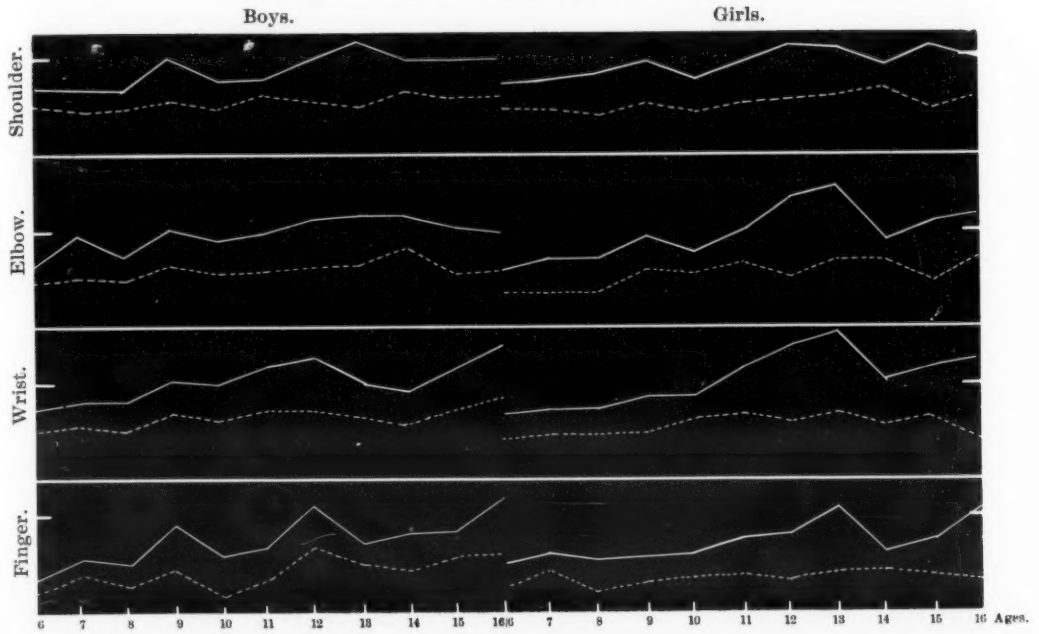


CHART III. RATE: BILATERAL ASYMMETRY.



THE TRAINING OF ANIMALS.

By James E. Le Rossignol, Ph. D.

A.—Dogs.

Professor Brewer of the Sheffield Scientific School has kindly loaned for the benefit of the writer the choicest of his invaluable collection of books on dogs. At the suggestion of Dr. Hall, the following notes have been prepared on these and several other works, and may be of interest to students of animal psychology.

Xenophon's Kynegetikus is the oldest known work on the subject. Among the Greeks different races of dogs were used for different kinds of game. For hares, Castorians and fox-dogs were used; for stag-hunting, Indian hounds, and for the wild boar, Indian, Cretan, Locrian and Laconian hounds. Chief attention is given to the coursing of hares. The dogs were not expected to catch the hare, but to drive it into nets which were set at certain places. The best dogs are those with a light head and blunt muzzle, prominent black shining eyes, broad forehead, long, flexible, round neck, broad chest, straight elbows. They must be strong, well-proportioned, swift of foot, and above all they must be keen-scented. Training must begin with young dogs. They should be kept in good health by nourishing food and plenty of exercise. The trainer himself should feed them, that they may become attached to him. When a pack is taken out to hunt, the older dogs are loosed first and then the younger are permitted to follow the scent with the others. They must usually be fed near the nets, that they may not wander too far from them, and they must be taught to drive the hare in that direction. If the scent be lost they must circulate until they find it again. As a reward the young dogs may be allowed to worry the hare. They are not to contract the habit of following foxes or any other animal than the hare. They should be taken out to hunt every two or three days, that they may become accustomed to the chase.

Arrian's Kynegetikus adds very little to what had been already said by Xenophon beyond recommending a few new races of hounds.

Of Englishe Dogges; a short treatise written in Latine by Johannes Caius, *and newly done into Englishe by* Abraham Fleming. London, 1576, pp. 44.

A curious book, in which the author, John Kay or Caius, gives his friend, Conrad Gesner, a description of the principal races of English dogs. It is interesting to notice that nearly all the chief races of the present time are mentioned by him.

Praktische Abhandlung von dem Leithund, CARL VON HEPPE. Angsburg, 1751, pp. 499.

No doubt at the time a valuable manual for huntsmen on the use of the led-hound. The hound was kept always in the leash and was used in tracking wild animals to their haunts. When once the game had been found a hunt was organized in the regular manner. The chief requisite for a led-hound is a keen scent. Training begins when the dog is but a few weeks old, and from that time he is in every way possible accustomed to the scent of the game he is intended to track. If it be deer, pieces of deer's flesh or skin are given him to smell and tear. When a little older he should be taken in the early morning to a place where the deer have lain during the night and encouraged to smell about and follow the scent as far as possible. Again, in a meadow a piece of warm bread or deer's flesh is trailed along the ground and the dog induced to follow the scent and allowed to eat what he finds. If he tries to find the scent in the wind his nose must be pressed down to the earth until he learns to sniff about on the ground. If he fails to follow the scent he is beaten, but if he does well he is caressed and rewarded. A tendency to chase birds or rabbits must at once be checked. He must be taught to love his master and always to obey his commands. He must be gradually accustomed to follow a weak scent by being kept at work until a late hour every day, when the sun has partially removed the scent. Restraining with the leash counteracts too great eagerness and prevents the dog from running over the scent, while words of encouragement and rewards tend to increase his ardor.

Ausführliche Geschichte der Hunde, ANON. Leipzig, 1781, pp. 374.

The author, who styles himself Kynophilus Actacon, begins his work with a chapter on the psychology of the dog. Dogs, as well as men, are either of choleric, melancholic, sanguine or mixed temperament. The chief faculties of the dog's *moral nature* are genius, instinct and memory. Genius is the power of quickly learning and comprehending anything. Dogs also possess the power of forming ideas, and exhibit attention and curiosity. The will is determined by innate tendencies or impulses, or in other words, by instinct, and forms the most important part of the dog's *moral nature*. Among the various impulses are desire for food, procreative impulses, impulse towards self-preservation, desire for amusement, exercise, fresh air, and for society. By means of these impulses the dog is impelled to eat, to mate, to hunt and fight and flee danger, to play and run and to associate with his kind and with men. It is by taking advantage of these impulses that man is able to train the dog in so many different ways. Dogs are trained by means of a judicious use of rewards and punishments, by the association of pleasant feelings with certain actions and painful feelings with others. The dog's own impulse towards self-preservation leads him to obedience, and when habits are formed rewards and punishments are no longer necessary. The training of dogs must be varied according to the ends to be attained, but the principles to be followed remain the same.

Der Hund, seine verschiedene Zuchten und Varietäten, DR. FRIEDRICH LUDWIG WALTHER. Giessen, 1817, pp. 96.

The chief characteristics which render the dog so useful to man are the keenness of his senses, his quickness of perception, his teachability, power of memory, faithfulness and obedience.

The General Character of the Dog Illustrated by Anecdotes, JOSEPH TAYLOR. London, 1821, pp. 150.

As with most anecdotes on canine sagacity, the lack of exactness and complete explanation of circumstances render it impossible to draw any safe conclusions from the instances here related.

Kunopædia; a Practical Essay on Breaking or Training the English Spaniel or Pointer, WILLIAM DOBSON. London, 1814, pp. 235.

The dog is to be gradually accustomed to obey certain commands by voice or gesture. Before being taken to the field the dog must learn to obey the commands, "come here" and "down." Obedience is rewarded by caressing, while the contrary is punished with the whip. After whipping, the master must make friends with the dog before letting him go, and he must never be chased, that he may not learn he is swifter than his master. The method used in the field is much the same as that given by Col. Hutchinson, except that more severe punishment is recommended. The great principle to be followed is that of complete obedience.

Canine Pathology, DELABERRE BLAINE. Lond. 1832, pp. 310.

This was the first scientific work on the subject and the author has been called the father of canine pathology.

The Dog, WILLIAM YOUATT. Philadelphia 1857, pp. 392.

A more complete work than the former and long the standard work on the subject.

The author thinks that in regard to intellectual powers the difference between the dog and man is in degree and not in kind. The brain of a man weighs about $\frac{1}{50}$ of the weight of his body, that of the Newfoundland dog $\frac{1}{60}$, while that of the bull dog weighs only $\frac{1}{300}$ of his entire weight. So great is the influence of heredity that instances have been known of dogs which at the age of a few months were self-broke and required no special training for the chase. If these induced habits, however, are not cultivated, they are soon lost. The pathology of the dog closely resembles that of man, both in the nature of the diseases and the mode of treatment. Between the mental conditions of the dog and of man in disease there would seem to be a striking parallel, especially in the case of hydrophobia.

The practitioner must understand the nature of the dog. He must be firm and courageous as well as kind and gentle in order to make the dog sensible of his power and of his good intentions. A sick dog often seems to realize that the doctor is endeavoring to relieve him, and will often lick the hand that for the moment causes him pain.

Vollständige Unterricht, alle Arten Hunde abzurichten, CHRISTIAN FRIEDR. GOTTL. THON. Ilmenau, 1832, pp. 296.

The trainer must be careful to select a dog of the proper race, size and temperament for the particular kind of hunting he is to learn. Still, the natural temperament of the dog may be greatly modified by careful training. The trainer must not only know the nature of the dog, but must exercise great care and patience. The dog must learn to know and love his master and the master must treat him kindly and never punish him too severely. Punishment should take place immediately on the commission of a fault, but should be in general by words rather than by whipping.

Training must proceed step by step from the simple to the more complex. Too much ought not to be attempted at once. Frequent repetition is necessary. As a reward for good behavior the lesson may be shortened, or it may be lengthened if the dog be lazy and disobedient. The lesson should take place when the dog is somewhat hungry, for then his faculties are on the alert.

Instruction must always be accompanied by words and the same words must always be used with the same meaning.

Dogs: Their Sagacity, Instinct and Uses, GEORGE FREDERIC PARDON. Lond. 1859, pp. 306.

The author defines instinct as "a faculty given to the lower animals instead of reason, a faculty that teaches them all they require to know, a governing principle that impels them to do all that is necessary for the preservation of their lives and the continuance of their species, and it operates without the aid of instruction or experience."

House Dogs and Sporting Dogs, JOHN MEYRICK, Lond. 1861, pp. 250.

A handbook intended for the use of dog-breakers. Methods of training must be adapted to the disposition of each particular dog. Some dogs are rendered vicious by whipping, while others require it to subdue their obstinacy.

The master should train his own dog. If training be carried on gradually and if each lesson be thoroughly learned before proceeding to the next, great progress may often be made in a short time. A retriever, for instance, is first taught to fetch and carry, then to follow the scent of a piece of meat that has been trailed along the ground, then to track a lame rabbit through long grass and to fetch it without injury, and finally to retrieve game in a similar manner. Many amusing tricks may easily be taught by taking advantage of some peculiar habit of a dog. If a dog be in the habit of standing on its hind legs for food, it can easily be taught to dance by holding food above its head. When two or three such tricks are learned, others are acquired much more rapidly.

Breeding, Training, Management, Diseases, etc., of Dogs, FRANCIS BUTLER. Brooklyn, 1879, pp. 391.

Almost every dog, the author thinks, is capable of a high degree of training, although some breeds, owing to heredity, seem more apt to receive instruction than others. Training should begin as early as possible, for it is much easier to teach right habits than to break off wrong ones. The dog should learn to obey from a desire to please his master rather than from a fear of punishment. Only one person, preferably the owner, should teach the dog, and a consistent system should be used, that the dog may not become confused at the different commands and methods. The same sign or word should always have the same meaning. The trainer should always show pleasure when an act is rightly performed and displeasure when a mistake is made. Advantage should be taken of the instinctive actions of dogs. Thus a puppy seems instinctively to run after things and carry them about, and a little training will teach him to do this at the word of command. Similarly, some dogs seem instinctively to take to the water, while others have an instinctive passion for hunting. Dogs have retentive memories and associations between commands and actions are easily formed, and thus a dog's *vocabulary* may gradually be enlarged.

Dog-Breaking, GENERAL W. N. HUTCHINSON. Lond. 1876, 6th Ed., pp. 348.

This book treats chiefly of the training of pointers and setters and is probably the best work on the subject.

The breaker should possess a thorough knowledge of dog natures. He should have perfect command of his temper and should use no severity. He should be consistent, that is, he should reward the dog for acting rightly, even when he has missed the game, and should not fail to punish a mistake even when the game has been bagged. He must act with reflection by considering what meaning the dog attaches to every sign and word and always using the same words with the same meaning.

Training should begin with young dogs at home. They must be taught to pay attention to the whistle and to obey certain words and signs. Of these, the word "toho," accompanied by raising the right arm, is the command to stand still; the word "on," with a forward under swing of the right arm, means that the dog is to advance in the direction indicated by the hand. "Drop," is the order to crouch. The orders should gradually be given in a low voice until finally the gesture alone is used. The dog must be accustomed to watch his master for commands. When these and several other commands have been well learned, the dog is taken to the field. He is expected to discover the bird by the taint in the air and not from the ground. Therefore he should enter the field at the leeward side. The trainer may use a check cord of from ten to twenty yards in length. On entering the field he sends the dog from him towards the wind by a wave of the hand or the word "on." The trainer makes the dog beat the field from side to side while he advances up the middle. When the dog has gone far enough in one direction, he is called back by a wave of the left hand or by a whistle, or, if necessary, he is pulled around by the check cord. As he passes, the trainer encourages him on in the opposite direction. When, by watching him, the trainer observes signs of his having scented a bird, he raises his right hand. If the dog does not stop the word "Toho" is given, and if he still refuses to stop the cord may be used and the dog pulled back to the place where he should first have pointed and there punished with constant repetition of word and sign until he obeys. If now the bird rises and the trainer fires, the dog must crouch at the word "down charge" and must remain in that position until his master has reloaded and given the word "seek dead," when the dog must find the game, yet is not allowed to take it in his mouth.

After a few weeks' practice with a careful trainer a dog will begin to acquire the proper habits of hunting. Much depends on the preliminary lessons, which should be thoroughly learned before more difficult feats are attempted.

The Dog; by Dinks, Mayhew and Hutchinson, FRANK FORESTER. New York, 1873, pp. 655.

A compilation of Dinks' book on the general management of dogs, Mayhew's Canine Pathology and Hutchinson's Dog-Breaking. Mayhew lays especial stress on the fact that the dog is essentially a nervous animal and that regard must be had to this both in the training of dogs and in the treatment of their diseases.

Die Hundezucht im Lichte der Darwinschen Theorie, GUSTAV LUNZE. Berlin, 1877, pp. 231.

Artificial selection and training by man have been the chief causes of the great variations now existing between the different

canine races. If left to themselves the different races would probably revert to a few types resembling some variety of wild dog.

Der Vorsteh-Hund, FRIEDR. OSWALD. Leipzig, 1873, pp. 290.

An exhaustive treatise on the training of the setter.

The American Kennel and Sporting Field, ARNOLD BURGES. New York, 1876, pp. 201.

The essential qualities of a good hunting dog, says the author, are nose, staunchness, pace, endurance, intelligence and high-breeding. These should all be present in a proportionate and well-balanced degree. All of these characteristics are hereditary. Also the particular methods of hunting are hereditary. The progeny of a pointer shows a natural tendency to point game. Highly bred dogs also as a rule show greater intelligence than others. Therefore, in breeding, both parents should be of good stock and well accustomed to hunting.

It is best for a sportsman to break his own dog, for the dog becomes accustomed to the sound of his voice and his general manner and becomes attached to his first master as to no other. Also dogs always hunt best the birds they are broken on. In both these cases first impressions are the strongest and most enduring.

No set of rules will apply to all dogs, and there is a wide field for the exercise of common sense.

The Dog; with Simple Directions for his Treatment, "IDSTONE." London, 1872, pp. 252.

This is a standard work on dogs, chiefly descriptive of the different varieties. The English fox-hound is adduced as a remarkable example of the excellency to be obtained by careful breeding.

The Dogs of the British Islands, "STONEHENGE." London, 1892, pp. 279.

The editor, one of the greatest authorities on dogs, has collected from various reliable sources accounts of all the principal canine races.

Le Chien; d'après les Ouvrages de STONEHENGE, YOUATT, MAYHEW, BOULEY, HAMILTON SMITH, &c. Paris, 1876, pp. 330.

A valuable and condensed compilation from the works of the standard authorities on dogs.

The Dogs of Great Britain, America and other Countries. New York, 1879, pp. 366.

Compiled from two works by Stonehenge and some chapters by American writers.

The description of the German Dachshund is suggestive of the very different modes of treatment which must be used with different dogs. "Dachshunds are head-strong and difficult to keep under command; and, as they are at the same time very sensitive to chastisement, it is next to impossible to force them to do anything against their will. Many good badger dogs have been made cowards for life by one severe whipping. They must be taken as they are, with all their faults as well as their virtues."

The Practical Kennel Guide, GORDON STABLES. London, Paris, New York, pp. 200.

Dogs; their Origin and Varieties, H. D. RICHARDSON. New York, pp. 127.

Dogs; their Points, Whims, Instincts and Peculiarities, HENRY WEBB. London, pp. 347.

The author thinks the bad reputation of the bulldog to be largely the result of his being in general chained up and trained for fighting, and thinks him capable of being taught anything as well as any other breed. Out of eight bulldogs owned by a certain gentleman, six learned to retrieve by land or water, and only one was at all bad-tempered.

Dogs and their Ways, REV. CHAS. WILLIAMS. London, pp. 376.

Especial attention is drawn to heredity of acquired habits. A puppy of a dog who had been taught to beg, at the age of seven or eight months spontaneously took to begging for everything it wanted. Other curious illustrations are given of similar heredity.

Natural History of Dogs, Vols. 18 and 19 of the Naturalists' Library, by COL. CHAS. HAMILTON SMITH, pp. 566.

Chiefly a description with plates of the various canine races, including the wolf and jackal.

A Guide to Dog-Training. New York, pp. 105.
Dog and Gun, JOHNSON J. HOOPER. New York, pp. 105.
Des Hundefreund par Excellence, MATTHIAS ALISTA. Wien. pp. 73.
Die Dressur des Hundes, ED. ZBORZILL. Berlin, pp. 202.

The author is a professional dog-trainer. Many young dogs, especially those of hunting breeds, very early show a tendency to fetch and carry, and every young dog likes to run about with things thrown to him. When a ball or glove is thrown to him he must be induced to bring it to his master at the word "fetch," and lay it down at his feet. If he will not let it go it must be gently disengaged from his teeth and placed on the ground at the proper place. Then a piece of bread or meat should be given to the dog, and he should also be patted and praised. On a second trial he will be more likely to lay the glove at his master's feet, and after a few lessons he will always do it so. Little by little the lesson may be varied and more required, until the dog will seek anything at the word "seek," or will learn to run errands with a basket.

There should be no whipping at first, but in every case something to eat should be given the dog when he has done anything properly, and he will soon learn to take pleasure in trickperforming.

The most difficult tricks are only combinations of very simple performances. The so-called counting and spelling by dogs is taught by mechanical devices aided by words and signs which the dog learns to associate with the action required.

A dog is taught to walk or dance on his hind legs by placing him in the position and aiding him with a stick, or it may be taught by holding a piece of meat above his head. He may be taught to turn round on the floor by tying a bit of meat to his tail. He will shut the door with his fore paws if a piece of meat be so held that he must climb against the door to reach it. In every case words of command are used and in time the dog will obey the word without the device that was necessary at first.

Researches into the History of the British Dog, GEORGE R. JESSE. London, 1866, 2 vols. pp. 385-424.

A most valuable work from a literary and historical point of view.

The History of the Mastiff, M. B. WYNN. Melton Mowbray, 1886, pp. 222.

The author is of the opinion that the English mastiff has from the earliest times existed in Britain, and is the ancestor of the bulldog.

Histoire du Chien chez tous les Peuples du Monde, ELZÉAR BLAZE. Paris, 1843, pp. 460.

A valuable work, dealing chiefly with the question of canine sagacity. The characters of dogs show much greater variety than those of wolves and foxes, because of long association with man. The dogs of civilized nations show much higher development of character than those of savages. The disposition of the male is also very different from that of the female. The character of a dog depends on the education he has received, on the kind of people about him and especially on the conformation of his skull. Education can do much to change the natural disposition of a dog.

A dog is essentially a creature of habit. What it is in the habit of doing that it does with pleasure.

There are two factors in a dog's mental development, his own experience and his education. A dog with a wide experience like a collie is more intelligent than one whose experience is confined to his kennel and his master's yard. A large circle of ideas implies greater intelligence. One has but to watch the behavior of an old and a young dog while hunting to see how much a dog will learn by experience.

The author says with regard to inherited instincts: "The dog is instinctively a hunter. We have adopted certain varieties to hunt for us, and they have become more apt than other dogs, but it is none the less true that all dogs are hunters by nature."

Histoire physiologique et anecdotique des chiens, BÉNÉDICT HENRY RÉVOIL. Paris, 1867, pp. 394.

A very complete work, descriptive of all the principal canine races. The author believes the dog to have been domesticated by man from the very earliest periods. Some of the oldest Chinese, Indian, Assyrian and Egyptian writings mention the dog as a domestic animal. The hunting instincts of the wild dog still survive in the hound, and the great ferocity often exhibited in the chase would seem a temporary recurrence to savage life and habits.

Le Chien; Histoire Naturelle, avec un Atlas de 67 Planches et 127 Figures, EUG. GAZOT. Paris, 1867, pp. 546.

A more scientific work than the last. The origin of the dog is unknown, although the fact that dogs when crossed with wolves and jackals produce a fertile offspring, seems to point to a common origin of all three families.

The influence of heredity and education on the physical and mental development of the dog is very great. Judicious crossing tends to the development of strength and intelligence, and too much in-breeding acts in the opposite direction. There is in dog a continual tendency to degenerate, which comes into operation whenever the care of man is removed, and a "reversion to type" is ever imminent.

The author refers to Darwin's observation on the training of sheep dogs in Montevideo. When quite young the dog is removed from its mother and placed with the flock, where its entire life is to be spent. Frequently immense flocks of sheep are left at a distance of ten kilometres from any habitation, entirely in the care of these dogs. The affection they show for the sheep is remarkable.

The great value of watch-dogs depends chiefly on two faculties, their acute sense of hearing and the restlessness they feel when they hear the least strange sound. By properly directing these faculties a good watch-dog is produced. In the training of dogs habits are to be acquired through a repetition of experience. Mental and physical qualities must be developed together. The natural instincts of the dog form the basis of training. Commands are given by voice and hand and the dog is accustomed to associate certain actions with certain signs, obedience with reward, and desobedience with punishment. The trainer should often himself do what he wishes the dog to perform. Thus when a shepherd trains his dog to lie down at the word "down," he lies down himself and makes the dog do the same.

Patience and time are always necessary, but the dog's natural instinct and intelligence aid in the speedy formation of habits, and with succeeding generations, owing to the power of heredity, the work is less difficult.

The Dog in Health and Disease, WESLEY MILLS. New York, 1892, pp. 407.

A complete and valuable work chiefly in regard to canine pathology, but also containing a chapter on breeding and training. Dr. Mills says: "A puppy, full of life, tends to do exactly as his impulses urge him, till the highest motive power, a desire to please his master, is substituted." Training, therefore, is a question of the direction of impulses by the formation of pleasant and painful associations, leading finally to the establishment of the habits desired. With regard to the methods to be employed the author is in accord with the best authorities.

ON THE JUDGMENT OF ANGLES AND POSITIONS OF LINES.

A.—On the Judgment of Angles.

By Joseph Jastrow, Ph. D.

With the assistance of Geo. W. Moorehouse, Fellow in Psychology.

The nature and extent of our errors in estimating and reproducing angles are the subject of our present inquiry. The point acquires a special interest from the fact that a number of writers have based their explanations of important optical illusions upon the view (apparently not tested by experiment) that acute angles are underestimated and obtuse angles overestimated. Such an investigation naturally begins with a definite mode of judgment or reproduction under definite circumstances, and is then to be supplemented by an investigation of the extent to which the results obtained are due to peculiarities of the method employed. We selected the angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165° as our standard angles and drew these upon circular pieces of cardboard (3 in. in diameter); the lines themselves were 30 mm. long and were so placed that one line was always horizontal. The papers on which the angles were to be reproduced by the subject were placed on a table and the drawings made in the normal writing position. For convenience all acute angles were formed on the left hand side and obtuse ones on the right, so that the drawing might be uniformly from left to right. The subject viewed the angle as long as was needed to fix it in his mind (from 5 to 15 seconds) and immediately thereupon, from his memory of the angle, drew another as nearly as possible equal to the first. He did this by adding a line to a horizontal line of 30 mm. which was given him (always in the same position) upon the squares of paper upon which he drew. The drawing was done with a hard, well-pointed lead-pencil. Each of the standard angles occurred twice in a set of 22 angles, the order of the angles being determined by chance. The results of the measurements of 62 such sets, or 124 reproductions of each angle by 13 subjects in all, are shown in the following table :

Standard Angle	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
Average Reproduction	18°03′	30°27′	42°09′	56°25′	69°44′	90°06′	113°34′	123°55′	136°29′	150°33′	163°49′
Error (from corrected standard)[1]	+2°23′	−0°56′	−2°16′	−5°13′	−6°46′	+0°06′	+7°54′	+3°22′	+0°19′	−1°05′	−2°41′

[1] For details of measurement and preparation of tables and curves, consult the note at the end.

It will be seen that the overestimations and underestimations can hardly be said to follow any simply formulatable law, such as the underestimation of acute and the exaggeration of obtuse angles; their full significance appears only in the curve as given below (Fig. 1); in this curve the differences between the actual and the re-

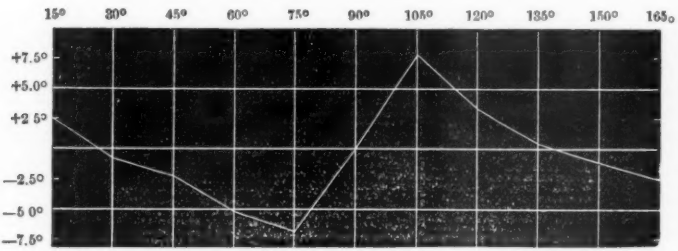


Fig. 1. Errors, in degrees, in reproducing angles by memory: Average of 13 subjects.

produced angles are plotted to the scale of one of the divisions to five degrees of error. This is our general result and its salient characteristics are, (*a*) the exaggeration of the angle at 15°, which passes into a gradually increasing underestimation up to 75°; (*b*) the correct reproduction of right angles; (*c*) the maximum exaggeration of the angle of 105°, which is followed by a decreased exaggeration, passing into an underestimation of very obtuse angles.

The next important inquiry is naturally how far the result is typical, and how far accidental; how far the result of the combination of different curves and how far the individual records agree with the general result.

By each of the thirteen individuals the angle of 15° is exaggerated; in all of the thirteen cases there is a falling off towards the next point 30°, the angle being about as frequently slightly overestimated as slightly underestimated; in all but two cases there is a fairly regular increase of the underestimation, reaching a maximum at 75°; in all cases the right angle is nearly correctly reproduced, the error being as often in one direction as in the opposite; in all cases the curve then sharply rises, reaching the maximum of exaggeration at 105°, and from there in eleven cases there is a more or less regular decline; the curve at the last point 165° falling below the line. Again, we may calculate the average deviation of the thirteen results from their mean; this for the eleven angles is 2° 31′, and of the 143 records (13 subjects for 11 angles) 87 show a deviation less than this average. Regarding the variation for the different angles it is least for 90°, the other angles following in this order, 150°, 165°, 15°, 30°, 135°, 75°, 45°, 120°, 105°, 60°.

Comparing the general outlines of the individual curves with the average curve, we find that in ten of the thirteen individuals, the correspondence is obvious and in most of these, striking; in one case the curve presents quite a different appearance, and in two other cases the differences are considerable. This diverging curve, however, is that of a professor of engineering, who has considerable experience in the estimation of angles; he draws angles of 30°, 90°, 135° and 165° very accurately, overestimates angles of 15°, 60°, 105° and 150° considerably, and underestimates the angles of 45°, 75° and

120°, thus presenting a zig-zag curve. The other two subjects came prepared with some practice in drawing and exhibited peculiarities in the mode of estimating the angles. In general there is thus a very striking similarity between the individual and the general result, so that the curve may be regarded as fairly typical for the average person.

The suggestion is close at hand that this result may be influenced by the mode of reproduction ; to test this, three of the subjects reproduced the angles, not from memory but with the standard angle constantly visible for comparison. The resulting curve is quite similar in the two cases, the essential difference being that the entire curve is closer to the true line of no error, i. e., the error is smaller. Fig. 2 shows the average result of the three subjects for each mode of reproduction.

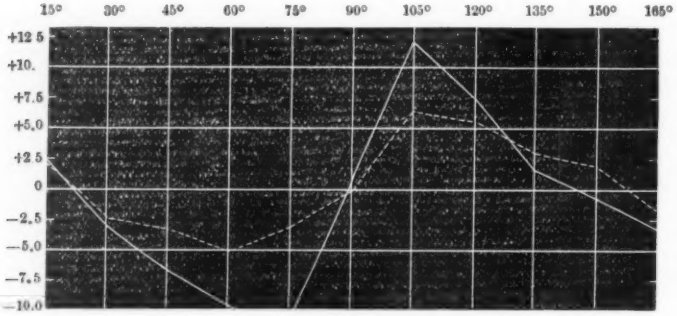


FIG. 2.

.....Errors in reproducing angles with both angles visible. Average of three subjects.
——Errors in reproducing angles by memory. Average of same three subjects.

It is likewise interesting to determine the regularity and accuracy of these methods of reproducing angles ; a sufficient test of this is the average deviation of the results from their mean. This is very troublesome to calculate and we have contented ourselves with doing it for two individuals. In reproducing angles by memory the average deviation of the one subject (fourteen judgments of eleven angles) was 2° 50′, of the other 3° 05′; in reproducing angles with the standard angle in sight 1° 33′ for the one and 2° 08′ for the other. There is thus indicated an increase of regularity with the decrease of absolute error. A comparison of deviations for the several angles shows that the right angle has by far the smallest deviation (about one-fourth of the average), and that the smallest and largest angles are somewhat more regularly reproduced than intermediate ones, and thus again indicate the direct relation between error and regularity.

Finally as to the significance of these results, we may offer the following suggestion. Our curve may be viewed as consisting of two portions, the first beginning with 15° and ending with 75°; the second beginning with 105° and ending with 165°, i. e., we omit the right angle as well as the angle 0°. In that case the curve falls into two (often strikingly) similar portions, beginning with an exaggeration and ending with an underestimation. This would mean that angles with a small excess over 0° or 90° are more exaggerated

or less underestimated than angles with a greater excess over 0° or 90°, and in this special sense is it true that acute angles are underestimated and obtuse angles overestimated; the smallest and largest angles forming an exception to the generalization.

More on Details of Method.—The angles were measured by applying a square of card-board 25 mm. square to the horizontal line, having the apex of the angle coincide with a corner of the square; the distance of the intersection of the oblique line with the side of the square was noted to the nearest ½ mm. and from this the tangent of the angle could be readily calculated; for the above process gave us the measurement of the opposite side of an angle whose adjacent side was always 25 mm. In this way under favorable circumstances a set of twenty-two angles could be measured and the results tabulated in five minutes. There is inevitably some error in this mode of measuring, and to eliminate such error, as far as possible, we measured our standard angle by the same method, finding as a result the angles 15° 40′, 31° 23′, 44° 25′, 61° 38′, 76° 30′, 90°, 105° 40′, 120° 33′, 136° 10′, 151° 38′, 166° 30′; the deviations plotted in the curves are from these angles and not from the theoretical angles 15°, 30°, 45°, 60°, 75°, 90°, 105°, 120°, 135°, 150°, 165°. In comparing the general average, the average of each individual was weighted by the number of sets of which it was the average.

B.—On the Judgment of the Positions of Lines.

With the assistance of James H. Turner.

Several points in the results just described suggested further research. The fact that one side of each angle was given as well as that the lines are drawn on square pieces of paper with one line parallel to the side of the square, may have important influences upon the results. To eliminate entirely the influences which these conditions may have induced, it seemed necessary to ensure an environment for the subject in which no straight lines whatever should be visible except those judged, for the lines of the floor and walls are manifestly sufficient to give him his vertical and horizontal and thus a basis for estimating angles.

To secure these conditions we arranged the experiment so that the subject could see nothing but one or two white card-board discs, four inches in diameter, upon which was drawn a straight black line three inches long. The two discs, one above the other, were viewed against a large black disc thirty inches in diameter, all placed in a vertical position. Above his head was a parasol-like frame, from which hung black draperies and a similar black cloth was drawn across his lap. When in position he was completely enclosed under this canopy, the light coming in from the back above his head; no portion of the floor or walls was visible to him. He was seated on a chair with his eyes on a level with a point midway between the two discs and about 15-20 inches away from them. The subject's arms reached outside of this canopy and held two handles attached by cords to the axle upon which the upper of the small discs turned. By pulling the right or left cord the subject could thus bring the straight line drawn upon the card-board into any position. This line was three inches long and 1 mm. wide and its centre was the centre of the four-inch disc. At the other end of this apparatus, which was firmly mounted upon a table, sat the observer, who had before him two circles divided to half degrees; to each of the other ends of the axles, upon one end of which was glued a four-inch disc, was attached a "hand" ending in a fine needle point, which was so ad-

justed as to assume precisely the same position as the line upon the disc. This adjustment was constantly regulated by setting the line vertical (by a fine plumb line) and noting the deviation, if any, of the hand from 90°. Finally a third axle midway between the two that bore the four-inch discs and thus in the centre of the thirty inch disc, bore a five-inch black paper disc eccentrically mounted and covering at pleasure either the upper or the lower disc.

A twisted cord attached to the axle and also to a hinged lever, the cord drawn and kept tense by a weight, enabled the operator by a simple movement to conceal either the upper or lower disc. Both were never in view at once. With this apparatus our method of experimentation is very simple. The operator sets the line of the lower disc at any desired angle; he then uncovers this disc, allowing the subject to view it until a clear impression of the position of the line is obtained; he then instantly covers this disc, and the subject, by means of the strings, sets the line of the upper disc to correspond to the remembered position of the lower line. The operator reads the position on his divided circle and the difference in the two readings gives the error. In the meantime another position has been set on the (invisible) lower disc, which is now revealed, and so on. Eighteen positions were used, forming angles of 0°, 10°, 20°, 30°, 40°, 50°, 60°, 70°, 80°, 90°, 100°, 110°, 120°, 130°, 140°, 150°, 160°, 170° respectively with the horizon. The observations were taken in sets of eighteen, each angle occurring once in a set. With this apparatus we studied the error in setting, after a brief interval, one line in the same position as a standard line; this judgment clearly involves angles, for it is based partly at least upon the angles formed with ideal verticals and horizontals. It also involves the conception of parallelism, for the task may be conceived as that of setting one line parallel to another.

Ten individuals were tested, seven of them drawing each position of the line ten times, and three of them each line twenty times. The average settings in degrees and tenths of degrees of each of the ten subjects for each of the eighteen positions of lines are exhibited in the following table. The Roman numbers indicate the different subjects; the upper line, the standard positions of the lines. The average of all is shown in the lowest line:

	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°	110°	120°	130°	140°	150°	160°	170°	180°
I.	9.6	23.5	31.7	37.8	49.0	60.8	69.7	78.7	91.2	104.2	113.7	122.5	131.7	140.9	153.7	162.0	171.5	178.5
II.	12.0	21.1	27.8	41.4	55.5	60.2	68.3	79.2	92.0	104.2	114.4	123.4	129.8	140.8	151.6	162.5	171.0	180.4
III.	10.2	19.9	29.1	41.9	52.0	61.4	74.1	83.8	91.2	103.6	111.9	124.8	131.9	146.6	155.9	167.2	176.2	181.4
IV.	9.6	18.4	28.5	40.5	47.9	55.9	63.9	71.8	90.6	105.2	111.9	121.1	131.2	144.9	153.7	162.5	174.4	180.7
V.	11.9	19.8	30.0	43.3	50.9	57.9	67.6	74.9	90.4	102.2	111.0	121.2	129.7	138.3	150.8	157.2	166.4	179.6
VI.	14.3	24.6	37.8	47.1	54.4	65.3	72.0	82.8	90.9	106.2	114.4	123.6	133.2	141.2	153.1	161.3	172.7	180.6
VII.	10.8	21.9	31.9	39.4	52.4	61.0	72.7	82.9	90.3	101.5	114.0	121.4	131.7	143.2	152.7	162.5	173.0	180.3
VIII.	9.5	24.3	31.6	37.8	49.8	60.6	70.0	82.7	90.8	99.2	110.2	117.1	134.2	141.4	148.7	162.9	174.5	181.7
IX.	9.2	21.1	33.5	44.2	53.6	62.5	73.0	81.0	89.6	100.7	114.8	123.4	132.5	141.8	155.6	161.2	170.3	180.2
X.	10.6	19.6	29.8	39.0	46.0	57.8	66.7	76.1	89.0	105.0	114.7	126.1	133.5	141.9	151.3	158.5	168.9	179.8
Average	10.77	21.42	31.7	41.24	51.4	60.34	69.80	79.39	90.6	103.2	113.1	122.46	131.94	142.1	152.91	161.78	171.89	180.32

If these averages be plotted in a curve similar to that drawn for reproduction of angles, a very irregular curve will result, presenting practically no constant characteristics. The errors vary with each angle and almost with each individual. All the angles are so nearly correctly reproduced that the order of their correctness seems almost accidental, although there are abundant indications that the vertical (90°) and the horizontal (180°) are more accurately reproduced than any others.

It is also true that the angles are rather more apt to be over-estimated than under-estimated, and that the obtuse angles are rather more over-estimated than the acute ones. Of the 180 records entered in the table, 138 are over-estimations and 42 under-estimations. Of the 90 records for angles of 90° and less, 55 are over-estimations and 35 under-estimations; of the 90 records for angles between 90° and 180°, 83 are overestimations and 7 underestimations. To this extent the characteristics of the former results reappear. As a very rough comparison of the errors in drawing angles from memory with one angle and the side of the other given, in doing this with both angles visible, and in judging positions of lines, it may be stated that the average error for all angles (without regard to their being positive or negative) of the three individuals whose records we have for all those methods are 3° 40′ in the first case, 2° 42′ in the second, and 1° 53′ in the third. The entire curve for positions of lines is thus nearer the line of no error than that for reproducing angles. It must be remembered that much of this resulting absence of error is due to the balancing of errors of opposite directions, particularly so with acute angles. The subject had, if anything, a smaller degree of confidence in the correctness of his reproduction of positions of lines than in those of angles, and had decidedly less confidence in the former than in the latter when these were drawn with both angles visible. An indication of the regularity of these reproductions is furnished by the average deviations of the individual reproductions from their mean. This for the three methods (average of all the eleven angles, eighteen positions) is for one individual 2° 50′ for angles from memory, 1° 33′ for angles with the standard angle visible and 3° 15′ for positions of lines; for another subject 3° 05′, 2° 08′ and 3° 49′. This would indicate the greatest variability, least regularity for the estimation of positions of lines, the least variability for reproduction of angles, with the standard angle visible, and an intermediate degree for reproductions of angles from memory. This order corresponds with the subjective feeling.

In general, then, we conclude that in the reproduction of positions of lines without reference to any but imagined coördinates, the absolute error is small; is on the whole an overestimation of the angle;[1] is greater with obtuse than with acute angles; while the individual variation of the results is rather large. No simply formulatable law is followed by the resulting curve of error; the errors varying irregularly with the angle and the individual.

B II.—On the Judgment of Horizontal, Vertical and Oblique Positions of Lines.

With the assistance of W. D. Brown.

It has already appeared that much of our perception of angles and positions of lines takes place by reference to an ideal vertical and horizontal which we constantly carry with us and have had

[1] This overestimation means that the upper end of the line was set too far to the right; this may be due to a greater dependence of the right eye in judging or in the adjustment of the right hand.

forcibly impressed upon us by the countless verticals and horizontals with which civilization has surrounded us. It would indeed be strange if this enormously extensive experience with right angles, verticals and horizontals should not have left its impress upon our psycho-physiological organism. We have had some evidence of it in the accuracy of judging right angles; and the importance of the subject led us to undertake the determination of the accuracy of this ideal vertical and horizontal. We did this with the apparatus above described, using only the upper disc. The subject simply set this disc until the line upon it appeared to him exactly vertical or horizontal. We also had him set it in a diagonal position 45°, with the vertical or horizontal, speaking of these as "left oblique" or "right oblique," according as the upper end of the line pointed to the left or right. Each set consisted of 20 settings, in which the four positions, vertical, horizontal, "left oblique" and "right oblique," occurred in a chance order.

Observations were made upon ten subjects, eight of whom set each line twenty-five times (five sets) and two of whom set each line fifty times (ten sets). In the following table appear for each subject the number of settings of each position, the resulting average for each position and the average variation of the twenty-five (respectively fifty) records from their mean value.

The last line of the table averages these results for the entire ten subjects:

Subject.	No.	Vertical.		Horizontal.		Right Oblique.		Left Oblique.	
	Obs.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.	Setting.	Variation.
J. J.	50	89° 26′	0° 31.7′	179° 29′	0° 34.4′	42° 02′	2° 29.4′	134° 58′	1° 52.5′
W. D. B.	50	89° 51′	0° 45.3′	179° 19′	0° 30.7′	44° 59′	2° 41.2′	135° 46′	2° 29.0′
F. S.	25	90° 40′	0° 49.2′	180° 16′	0° 35.8′	42° 49′	2° 52.2′	138° 41′	3° 12.1′
F. E. B.	25	90° 42′	0° 40.1′	180° 39′	0° 54.7′	36° 25′	4° 04.4′	141° 25′	3° 58.2′
E. P. S.	25	92° 35′	0° 34.2′	182° 14′	0° 34.1′	44° 08′	2° 36.7′	139° 44′	1° 22.3′
J. H. D.	25	90° 26′	0° 25.5′	179° 52′	0° 51.4′	41° 16′	2° 04.8′	139° 20′	2° 28.2′
J. H. T.	25	88° 53′	0° 36.9	178° 52′	0° 57.5′	38° 40′	2° 52.1′	137° 40′	2° 59.0′
C. M. R.	25	90° 02′	0° 31.2′	180° 13′	0° 36.2′	41° 17′	4° 07.3′	146° 12′	3° 37.2′
G. W. M.	25	90° 09′	0° 30.4′	180° 02′	0° 24.1′	42° 39′	3° 16.6′	136° 42′	4° 09.0′
E. T. V.	25	89° 40′	0° 33.8′	180° 13′	0° 31.4′	33° 41′	2° 08.9′	141° 29′	3° 16.1′
Average.		90° 14′	0° 35.8′	180° 07′	0° 39.0′	40° 50′	2° 55.4′	139° 12′	2° 55.4′

It appears at once that the ideal verticals and horizontals that we carry with us are exceedingly accurate. This is shown not alone by the close approximation to 90° and 180°, but by the very small average variation, less than two-thirds of a degree. The individual variation is also small; 40.8′ for 90° and 36.1′ for 180° amongst the ten subjects. The diagonal positions show a larger and more constant error; the right oblique, if exact, should be at 45°, but, in all cases, it is less than this. The left oblique, if exact, should but be at 135°, but in all cases [in the first line (J. J.) there is an underestimation of 02′] it is overestimated. This means that in both cases the oblique lines were placed too near the position of the horizontal, in the one case (right) by 4° 10′, in the other (left) by 4° 12′. The average variation is also larger than in verticals and horizontals, being nearly 3°. The individual variations for the ten subjects are 2° 42′ and 2° 26′.

No elaborate comment upon these results is necessary. They give evidence of how thoroughly we have been drilled in the perception of rectangular coördinates; and the small variation both of the individual records and of the subjects is especially noteworthy. Our perception of positions midway between the vertical and horizontal is not so accurate nor so constant, the tendency being to approximate them too closely to the horizontal. The error is the same in direction and extent as that for angles of 45° and 135°, when they were reproduced with the standard angles visible (see Fig. 2 above), but is out of relation to the corresponding reproduction of positions of lines.

A Further Study of Involuntary Movements.

With the assistance of Thomas P. Carter and Edward P. Sherry.

In a previous contribution (this Journal, Vol. IV. pp. 398-407) there was described an apparatus—the automatograph—by which involuntary movements in the direction of the attention could be readily recorded; and typical illustrations were given of such movements, obtained under various conditions. In further study of these movements we attempted to determine the effect of the position of the body upon them, to analyze them with their constituent factors, and to experiment upon certain other points closely related to these.

If the arm be extended to the side of the body, movements of the hand forward are more readily made than movements backward, and movements toward the body more readily than movements away from the body. The hand moves most easily along a circumference of which the shoulder is the centre. The desideratum is a position in which movements in all directions would be equally easy; while this is almost impossible to secure, it may be approximated by extending the hand at an angle of about 45° with the line joining the shoulders and with the elbows bent at an angle of about 120°. The hand thus extended is placed upon the centre of the automatograph—firmly fixed to the table—and a constant position is secured by outlining in chalk the position of the subject's feet upon the floor. In this way the differences in question were reduced, but not eliminated; the average of all the comparable records at our disposal shows half again as extensive a movement towards an object of attention to the front as towards one to the rear, and a third again as much movement towards as away from the body. In some cases, too, the tendency to move forward overbalances the tendency to move towards the object of attention; in such cases we should have, however, a smaller movement to the front than when the object of the attention was to the front. In brief, the difference in the records accompanying the direction of

the attention to the front and to the rear, seldom fails to appear, although it may appear as a difference in the amount of movement instead of in the direction of movement. Fig. 5 shows a case of the former kind, obtained under different but comparable conditions; Figs. 7 and 8 illustrate the more usual result. The fact that the movements recorded serve as an index of the direction of the attention may thus be established independently of the influence of the position of the body; a conclusion corroborated by results to be described presently.

Observation of the subject's movements during an experiment strongly indicated that the result was complex, and originated in several portions of the body; it seemed both general and local. The chief factor in the general movement was referred to a swaying of the body with the feet as a pivot; this swaying of the head we recorded by fixing the recording plate horizontally on the subject's head[1] and suspending above it the glass pencil, held in an adjustable arm, which was firmly fixed to an upright on the table. The device for holding the writing point is the same as that used in the automatograph and is shown in full size in Fig 3. A cork (C) is pierced by the snugly fitting glass tube (T) and within the tube the pointed glass rod (R) moves easily up and down, accommodating itself to all irregularities of surface or movement; a small rubber band (B) is useful in raising the pencil off the record and in preventing it from falling through the tube. For the record nothing is better than the small ground glass drawing-frames that children use. These nicely hold and stretch the glazed paper; they may be stacked without injury to the record, and the frame prevents the pencil from leaving the record. This device may be variously used and may be recommended as the simplest method of recording movements of the kind in question. It is to be noted that when the subject holds the record-plate and the pencil is fixed, there is recorded a movement in opposite direction to that really made.

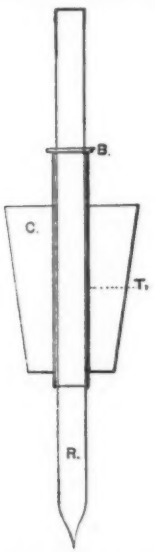


Fig 3. Device for recording movements. The glass rod (R) moves freely up and down in the glass tube (T) held in the cork (C). The rubber band (B) prevents the rod from falling throught the tube.

The movements of the head[2] show the influence of the direction of the attention similarly to those of the hand; indeed the correspondence between the two is considerable and often striking. It appears best when movements of the head and of the hand are recorded at the same time. Fig. 4 may serve to indicate the degree and nature of the correspondence; the head movements are apt to be more extensive and distinctive than those of the hand. This favors the conclusion—to be reinforced by other considerations—that the swaying of the body contributes an important part to the automatograph records.

[1] To fasten this upon the subject's head, a screw eye is fastened to each end of the frame holding the smoked paper; a rubber band is drawn over each arm up to the shoulder; to this band is affixed another rubber band passing through the screw eye and thus securing the frame upon the head. Some soft padding under the frame is desirable.

[2] The records for this study (Figs. 2-12 and 16) have all been taken upon the same individual and thus are as comparable as possible.

We failed to discover any constant tendency to sway in a special direction; movements backward seemed to be as readily made as those forward, and to the right as readily as to the left. When the attention is not directed in any special direction or is directed to a point overhead, an irregular forward and backward as well as lateral swaying results, which is quite different from that accompanying a definite direction of the attention.

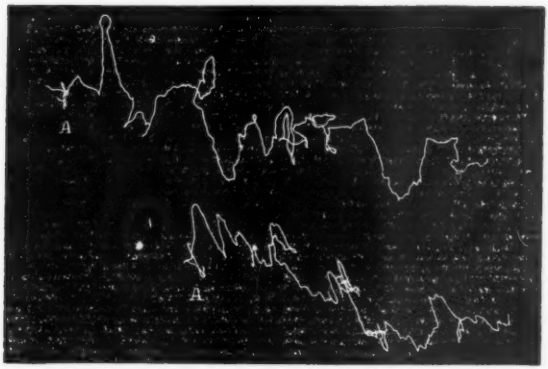


Fig. 4. Counting metronome. ⟶ Upper line, movements of head; lower line, of hand on automatograph; time, 45 seconds. The head movements are reversed, but have been again reversed for readier comparison. Figs. 4 to 13 are all obtained upon the same subject. The arrows indicate the direction in which the object attended to was situated.

The most obvious method of eliminating these swaying movements is to experiment with the subject in a sitting position. A typical record of the hand movement on the automatograph with the attention directed to the front appears in I, Fig. 5. The irregu-

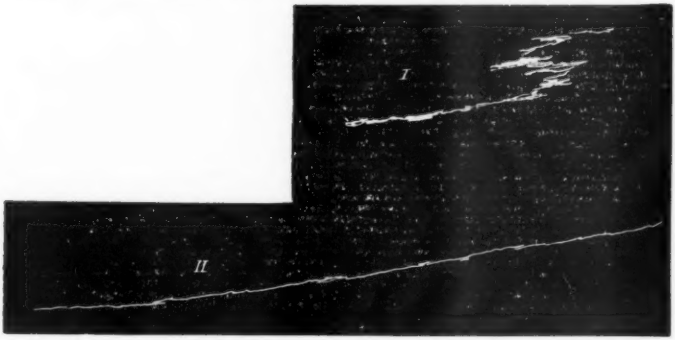


Fig. 5. Counting metronome. Facing ⟶. Automatograph, sitting. I, ⟵; time, 105 seconds. II, ⟶; time, 45 seconds.

lar lateral oscillations have nearly disappeared; the tendency to move along a circumference of which the shoulder is the centre is marked. A more satisfactory method of eliminating the swayings of the body consists of holding the pencil in one hand and the record-plate in the other; in this way pencil and record sway together and thus no record of it is made. Under these conditions we obtain a characteristic type of movement; the several oscillations are small and fine, as appear best when examined with a magnifying glass. Fig. 6 illustrates the type of movement very

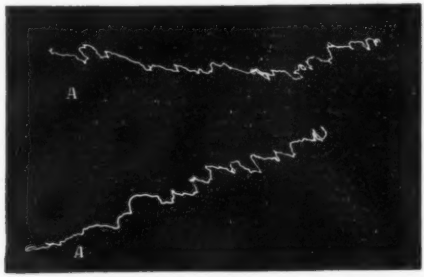


Fig. 6. → Counting metronome. Right hand holds pencil, left hand holds record; time of each, 90 seconds. Facing →. Upper line, standing; lower line, sitting.

well. It further illustrates that by this method there is no difference, or but a slight one, between the records taken while the subject is standing and while sitting; which is precisely what should be the case if the general movements of the body have been eliminated. In this figure traces are observed of a somewhat regular periodic "curve"; these mark the respirations, and in II, Fig. 7, they are sufficiently distinct and regular to be counted, about twenty to the minute. It is natural that the respiration should appear, because the arm holding the record-plate is rested against the body, and thus records the abdominal movements, though these are apt to be obscured by involuntary movements of the hand holding the pencil. The tendency to move towards the object of attention appears throughout; Fig. 7 further illustrates a movement to the rear as well as to the front, and in Fig. 8 we have an unusually clear indication of readiness with which the direction of the attention may be received. The subject attends to and counts the beats of a metronome, which is in turn carried from one corner of the room to the next; the hand accurately follows the attention, yielding an almost perfect square. In all these tests it is important that a position be chosen in which movements in all directions are equally possible.

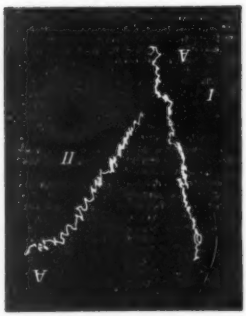


Fig. 7. Thinking of a building. Facing ↑; standing. Right hand holds pencil, ↑ left hand holds record; time of ↑ each 60 seconds. I, ↑; II, ↓; shows respiration.

As a further test of our analysis of these movements we recorded the movements of the two hands at the same time. This may be done by holding a pencil in each hand over a record-plate placed upon a table, or by holding a record-plate in each hand under a writing point projecting from a table or upright. The difference between these methods is not great; the former method allows of slight finger movements, while the latter does not. The latter is, on the whole, more convenient, because the natural sinking of the hand cannot spoil the record, which might be the case in the other method. The record-plate was placed upon a light board, to which a handle set vertically or horizontally could be attached. Both methods admit of a variety of positions of the arms and hands and dispense with the necessity of maintaining the record-plate level[1]. The results show that the movements of the two hands are very similar indeed; part but not all of this similarity is due to the swaying of the body, which would naturally affect the two sides alike; but there seems also a tendency for the two hands to move together in following the direction of the attention. Fig. 9 illustrates the close similarity of the movements of the two hands. It is important to remember that the records must be made in the same way and the hands be held in the same position. The tendency to move is greater when the hand is held away from than when held close to the body; Fig. 10 illustrates this difference and at the same time shows the correspondence of the form of the movement, notwithstanding the difference in extent.

Fig. 8. Counting metronome. Right hand holds pencil, left hand holds record. From A to B, ↑; from B to C, →; from C to D, ↓; from D to E, ←; standing. Each part, 45 seconds.

We have thus illustrated a variety of methods of recording involuntary movements and of analyzing the chief factors contributing to the result. In a measure this separates the mechanical from the psychological portions of the movements and sheds some light upon the positions and methods used in muscle-reading; the additional facilities derivable from the movements of locomotion should not be overlooked.

To this account may be added a few illustrations interesting from various points of view. Involuntary movements are naturally not limited to the horizontal plane; the rod sliding within the tube simply records these alone. We may fix the record-plate in a vertical position against the wall and take the cork between the fingers of the outstretched hand, holding the tube in a slanting position, and thus record vertical movements. This is, however, a fatiguing position, and the fatigue is manifested in a sinking of the hand and arm. This is usually quite rapid and may readily be

[1]The difference between records made with the automatograph and with the device figured in Fig. 1 held in the hand, is mainly one of extent of movement. The automatograph records more finely the tendency to move towards the object of attention as well as the general movements of the body. The illustrations of this article compared with those of the former show the nature of the difference. A further advantage of the automatograph is that it rests the arm.

Fig. 9. Facing ↑. Thinking of a building. Standing ↑. I, left hand; II, right hand; both holding record near the body; time, 35 seconds; records reversed.

Fig 10. Facing ↓ far out. Thinking of a building. Facing ↑ I, left hand held extended II, right hand held close to body; each hand holds record; time, 35 seconds; records reversed.

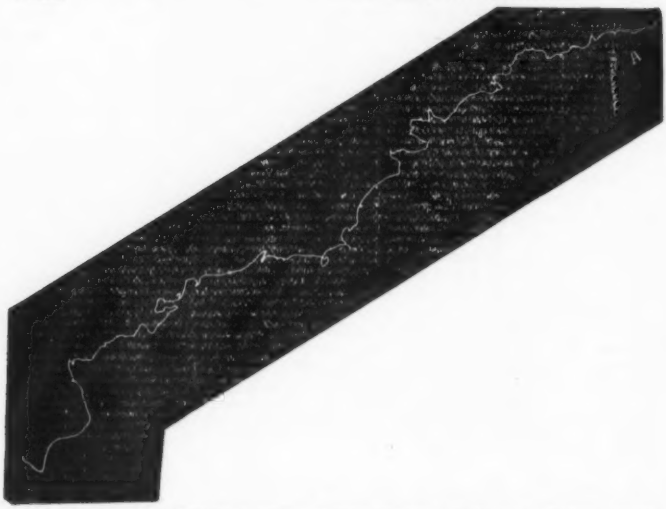


Fig. 11. ⟵ Counting metronome. Record vertical. Facing ⟵. Time, 20 sec. Pencil held in extended right hand.

recorded; an illustration is given in I, Fig. 12. If in this position the attention is directed forward, we obtain a resultant of the two tendencies, as is shown in the diagonal curve of Fig. 11. Fig. 12

further illustrates an interesting point similar to that illustrated in Fig. 5. In curve I the attention is directed downwards, which quickens, though probably not considerably, the natural tendency for the hand to fall; in curve II the attention is directed to a point overhead, and we observe that this tendency almost exactly balances the effect of the natural fatigue and thus yields this peculiarly interesting result.

Fig 12. I, thinking of ↓ Record-plate vertical. Thinking of one's feet; time, 45 seconds. II, ↑ a point overhead; time, 45 seconds.

Our attempts to utilize this method for measuring the different degrees of attractiveness of different senses or sense-impressions have not been very successful; and this is mainly due to the great

variability of the result. We have a few illustrations of the difference in question of sufficient interest to reproduce. In Fig. 13

Fig. 13. ⟶ I, counting metronome. Automatograph, facing ⟶. Time, 35 seconds. ⟶ II, counting pendulum. Automatograph, facing ⟶. Time, 25 sec.

curve I represents the movement of the hand while the subject was counting the strokes of a metronome for 35 seconds; the movement is towards the object of attention, but is slight. Curve II represents the movement where a pendulum is substituted for a metronome, a visual for an auditory impression. In this case the usual impression claims the attention more strongly than the auditory; and this corresponds with the subject's analysis of his mental processes. The subject is a noted American novelist and describes himself as a strong visualizer and in general an eye-minded person.

Fig 14. ⟶. Facing ⟶. Hand on automatograph. From A to A′; reading colors; 35 seconds. From A′ on, counting pendulum, 25 seconds.

In Fig. 14 the subject was asked to call the names of patches of colors fixed to the wall opposite him; he did this uncertainly for 35 seconds, his hand moving from A to A′; at this point the counting of a pendulum was substituted for the reading of the colors with a markedly different result, the hand moving directly and rapidly towards the pendulum. Upon examination it proved that the subject's color vision was quite defective, so that the colors did not hold his attention, while the pendulum did. The difference is too marked to be accidental, and is certainly most interesting.

An interesting problem upon which further research is contemplated is the correlation of types of involuntary movements with temperament, age, sex, individual, health and disease, and the like. A few observations were made upon children; they show at once the limited control children possess over their muscles and a similar difficulty in fixing the attention as required. They thus yield an exceedingly irregular result, showing very extensive and coarse

movements, usually towards the object of attention, but with great oscillations. Fig. 15 may serve as a type; in 35 seconds the hand

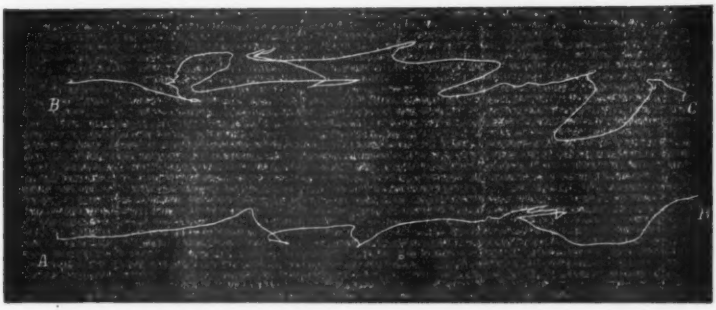


Fig. 15. ⟶ Hand on automatograph. Facing ⟶, counting pendulum. Time, 35 seconds. The record from B′ to C is continuous with that of A to B. The subject, a child of eleven years.

has moved by large skips *seven* inches toward the pendulum, the oscillations of which the child was counting. The difference between this record and those obtained upon adults is striking enough to warrant further study.

Much attention has recently been paid to the subject of automatic writing; in this the subject unconsciously gives indication not of the direction but of the nature and content of his thoughts. We made a few attempts to obtain such upon the automatograph, but entirely without success; we asked the subject to think of a certain letter or simple geometric figure and examine the record for any trace of the outline of this letter or figure. While this method yielded no trace of such a result, it gave us a valuable "control" over the movements in which the attention was directed in a definite direction. In Fig. 16 the subject was thinking of the

Fig. 16. Thinking of letter O. Pencil in hand; record on table. I, standing; II, sitting.

letter O; this was not thought of as in any special location and the record likewise fails to show a movement in any special direction. Two records are shown, one while standing and one while sitting, and these will show the difference in the general and local movements in the two cases; the subject is the same as in Figs. 4-12. This adds the corroboration of a negative proof to the general interpretation of the results.

Further Studies of Classification-Times.

With the assistance of Geo. W. Moorehouse, E. C. Bolton and E. T. Johnston.

This term was applied in the last series of these studies (this Journal, Vol. IV. p. 411) to the time occupied in referring a word or object to its class. The special problem studied related to the time required for answering by the word "noun," "verb" or "adj." (for adjective) when one of ten known nouns, verbs or adjectives was called; i. e., to refer a familiar word to its proper part of speech. This was done when the words were either *nouns or verbs, nouns or adjectives, verbs or adjectives, and nouns, verbs or adjectives.* The times were measured by aid of a "mouth-key," in which the release of a bit of wood held between the teeth started or stopped a chronoscope. The natural opening of the teeth in speaking thus served to mark the limits of the time measured. A simple reaction was obtained by answering always with the sound "*eh*" (violent explosive) when the voice of the observer was heard; the action of repeating the word heard was regarded as involving essentially all the steps involved in the classification-time, except that needed for mentally making the classification. In order to bring our former results in direct comparison with those about to be described, we repeated the same set of reactions upon F. E. B. and E. T. J. as were last year made upon the other two observers. The following table gives (in $\sigma = .001$ second) the simple reaction-time; the repetition-time; the classification-time of nouns and verbs, of nouns and adjectives, of verbs and adjectives; the average of these three; and the classification-time of nouns, verbs and adjectives. Each entry represents the average of about 12 sets of 20 reactions each. The lowest line gives the general average of all the results.

Subject.	Simple Reaction.	Repetition.	Noun-verb.	Noun-adjective.	Verb-adjective.	Average of N.-V., N.-A. and V.-A.	Noun-verb-adjective.	Difference between Simple and Average of N.-V., N.-A. and V.-A.	Difference between Repetition and Average of N.-V., N.-A. and V.-A.	Difference between Average of N.-V., N.-A. and V.-A., and N.-V.-A.
J. J.	190	367	599	595	593	596	667	406	229	71
G.W.M.	195	280	628	597	679	635	678	440	355	43
F. E. B.	201	364	579	623	604	602	663	401	238	61
E. T. J.	179	366	612	619	611	614	684	435	248	70
Aver.	191	344	604	608	622	612	673	421	268	61

From this table we may gather that it takes 191σ to signal by a vocal reaction that a sound has been heard; 344σ to repeat a spoken word; the difference between these 153σ for the processes of distinguishing the sound and calling into play the proper vocal utterance; 612σ to refer a spoken word to one of two grammatical

classes, there being no difference amongst the three pairs of parts of speech used; 673σ or 61σ additional to do this when the spoken word may be one of three classes of ten each; and 268σ for the mental process of deciding and recalling what the appropriate part of speech is.[1]

Our next step was to substitute for the grammatical classes the three classes of *animals*, parts of the human *body*, and articles of *dress;* as before, ten distinctive and familiar monosyllabic words were chosen as follows: *dog, fox, hen, pig, cow, bee, snake, goose, goat, horse; eye, ear, nose, mouth, head, arm, hand, foot, tooth, thumb; hat, cap, coat, vest, glove, shoe, boot, tie, cuff, shirt.* We answered "*an,*" when the name of an animal was spoken; "*bod,*" for parts of the body; and "*dress,*" for articles of dress. As before, we classified these words when they were selected from *animals* and parts of the *body*, from *animals* and *dress*, from *body* and *dress*, and from *animal, body* and *dress;* and in all respects repeated for these classes what he had done for the grammatical ones. Our results are embodied in the following table, in which each entry is the average of 12 sets of 20 reactions each:

ANIMALS, BODY, DRESS.—*Verbal Series.*

Subject.	Animal-body.	Animal-dress.	Body-dress.	Average of A.-B., A.-D., B.-D.	Animal-body-dress.	Difference between Simple and Average A.-B., A.-D., B.-D.	Difference between Repetition and Average of A.-B., A.-D., B.-D.	Difference between Average of A.-B., A.-D., B.-D., and A.-B.-D.
J. J.	747	768	693	736	809	546	369	73
G. W. M.	567	575	551	564	637	369	284	73
F. E. B.	724	709	683	705	783	504	341	78
E. T. J.	658	694	673	675	737	496	309	62
Aver.	674	686	650	670	741	479	326	71

Several conclusions drawn from the former table reappear in this one. The three kinds of classifications with two classes take about equal times; the additional time needed to make the same classification when the word may be one of three classes is about the same. Further, the process of classifying words as *animals, body* or *dress* is longer than classifying them as parts of speech for three subjects, for J. J. by 140σ, for F. E. B. by 103σ, for E. T. J. by 61σ, but 71σ shorter for G. W. M. G. W. M. experienced

[1] It is also noted that our additional research entirely confirms the conclusions drawn in the last study, viz., that the results for J. J. are more typical than the others. The peculiar difficulty of G. W. M. to distinguish verbs from adjectives still affects the results. But the main irregularities in our former results, due to irregularities of practice, do not reappear, owing to the fact that care was taken that the various kinds of reaction be equally distributed throughout the work.

unusual difficulty in naming the parts of speech of words, and found the classification with *animal, body, dress* somewhat easier than the others; it is, perhaps, fair to regard the average of the other observers 102σ (or an increase of 17%) as representing the increased difficulty involved. It is easier upon hearing the word dog, to recall and say that *dog* is a *noun* than that *dog* is an *animal;* which would in turn indicate that we have been better schooled in recognizing the parts of speech of words than in recognizing the more or less natural classes into which the objects denoted fall.

Our three groups of ten words each were chosen with reference to easy pictorial illustration, for our design included the classification of the pictures of these objects as well as of the spoken words. For this purpose pen and ink drawings of the thirty objects were prepared and fastened upon small slips of glass; the drawings were of a uniform size, the extreme outlines being contained within a circle 35 mm. in diameter. A frame was made in which ten of these pictures could be mounted in a carriage and moved along horizontally in back of a shutter such as the photographers use. This shutter consisted of two wings in back of an opening of adjustable size; a pressure on an air bulb withdrew the wings from the opening in the usual way, and in so doing established an electric circuit by which the chronoscope was started. By the aid of a series of spring stops and a weight to move the carriage, we could conveniently and quickly bring any one of the ten pictures behind the opening in the shutter. The reaction was made as before by speaking the appropriate class name with the mouth-key. After each ten reactions the pictures were changed and two series of ten reactions each constituted a set. The following table, arranged similarly to the former tables, gives the results of our "pictorial" series. Each entry under "simple" and "naming" represents the average of 12 sets of 20 observations each; each entry under the other columns of 8 such sets.

ANIMAL, BODY, DRESS.—*Pictorial Series.*

Subject.	Simple.	Naming.	Animal-body.	Animal-dress.	Body-dress.	Average of Animal-body, Animal-dress, Body-dress.	Animal-body-dress.	Difference between Simple and Average of A.-B., A.-D., and B.-D.	Difference between Average of A.-B., A.-D., and B.-D. and A.-B.-D.
J. J.	202	484	524	526	570	540	603	338	63
G. W. M.	214	522	547	494	532	526	605	312	79
F. E. B.	235	563	563	561	577	567	639	332	72
E. T. J.	185	558	545	507	524	526	589	341	63
Aver.	209	532	545	522	551	539	608	330	69

Again it appears that the classification of a word into any one of the three pairs of classes requires about equal times, and that the

additional time to do this for one of three classes remains the same, about 70σ.

The chief result of a comparison of this with the former table is the fact that it takes *less time to classify a picture than a word;* less time to recall and say that "dog is an animal" when the picture of a dog is shown than when the word is spoken. While the classification in the verbal series (for two classes) requires 670σ, in the pictorial series it requires only 539σ, or 131σ less; for classification into three classes, 741σ and 608σ, or 133σ less. It is fairer to take account of the differences in the simple reaction-time of the verbal and pictorial series, 191σ and 209σ; and thus the difference in the mental processes of classification is greater by 18σ than the differences just given.

In the verbal series we found reasons for regarding the time of repeating a spoken word as involving all the processes of classifying the word except the act of recalling the classification (see Vol. IV. pp. 412–413); the pure mental classification time for the grammatical series (two classes) would thus be 268σ, for the verbal animal-body-dress series 326σ. In the pictorial series we were unable to devise any means of making this elimination, and so cannot say how much of the difference between the simple reaction and the entire classification-time, 330σ, is taken up by the process of recognizing and indicating the recognition of the picture, how much by the recalling of its class name. There are strong reasons for believing that very much the greater portion of the 330σ is taken up in the mental classification process.

It is further of interest to compare the process of classifying with that of naming. Is it a more complicated process, upon seeing the picture of a dog, to say "dog" or to say "animal"? Do we first recognize the lines as representing a dog, and then decide that a dog is an animal, or do we at once recognize the drawing as that of an animal? We are able to give but an imperfect answer to this question. For J. J. it is easier to name than to classify, and the time is shorter by 56σ; for G. W. M. and F. E. B. there is practically no difference; for E. T. J. naming requires 32σ longer. The inferences from these results are that the two processes are about of equal complexity, that it is unlikely that the specific recognition of the class to which the object belongs includes the recognition of the individual object, and that the processes may be different in different persons.

We have already noted that the three pairs of distinctions of classes are of equal difficulty; it is further of interest to ascertain whether it is easier to pronounce a word a noun, verb or adjective; a name, a picture, that of an animal, of a part of the body, or of an article of dress. The following table gives the data for this decision, and by noting the numbers in bold type we see that on the average of all cases in which a word was pronounced a noun, the time required was 616σ, for verbs 627σ, for adjectives 651σ; for animals 695σ, for body 690σ, for dress 698σ; for pictures of animals 555σ, for body 568σ, for dress 564σ. In each class the three types of classification are thus of practically equal difficulty. The increase of time needed to pronounce a word an adjective above that needed to pronounce it a noun is of note, however, amounting to 35σ in the average and appearing in each of the four individuals. The individual records agree well with their average. The table will doubtless be clear without further comment:

Subject.	Noun.				Verb.				Adjective.			
	N.-A.	N.-V.	N.-V.-A.	Aver.	N.-V.	V.-A.	N.-V.-A.	Aver.	N.-A.	V.-A.	N.-V.-A.	Aver.
J. J.	604	594	631	610	594	568	651	604	596	618	698	637
G. W. M.	620	568	657	615	636	669	675	660	627	689	701	672
F. E. B.	594	626	610	610	564	604	652	607	620	604	728	651
E. T. J.	609	612	665	629	615	613	683	637	626	607	704	646
Average.	607	600	641	616	602	613	605	627	617	629	708	651

Subject.	Animal.				Body.				Dress. (Words.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B. D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	744	764	806	771	750	687	806	748	772	701	814	762
G. W. M.	566	547	628	580	568	549	632	583	603	555	651	603
F. E. B.	723	739	743	735	725	683	804	737	679	683	803	722
E. T. J.	657	680	742	693	659	667	746	691	708	679	723	703
Average.	674	682	730	695	675	647	747	690	690	655	748	698

Subject.	Animal.				Body.				Dress. (Picture.)			
	A.-B.	A.-D.	A.-B.-D.	Aver.	A.-B.	B.-D.	A.-B.-D.	Aver.	A.-D.	B.-D.	A.-B.-D.	Aver.
J. J.	521	517	581	540	527	569	619	572	535	571	610	572
G. W. M.	557	501	592	550	537	514	620	564	485	550	592	542
F. E. B.	561	562	622	582	567	576	635	593	560	578	661	600
E. T. J.	554	495	592	547	536	528	589	551	519	522	587	543
Average.	548	519	597	555	542	547	616	568	525	555	612	564

The final average (532?) represents the average of 960 reactions, animals being named 340 times, parts of the body 266 times, and articles of dress 354 times. We also append a statement of the number of errors, i. e., in which a word or picture was referred to a class not its own. Our data for nouns, verbs and adjectives relate only to F. E. B. and E. T. J.; for these the numbers of errors in percentage of observations taken are as follows:

Naming Pictures of Objects.

Subject.	Animal.	Body.	Dress.	Average.
J. J.	488	469	494	484
G. W. M.	530	513	524	522
F. E. B.	573	569	546	563
E. T. J.	557	558	558	558
Average.	537	531	527	532

It is also true that it takes practically the same time to name a picture of an animal, a part of the body, or an article of dress. This appears in the following table:

Subject.	N.-A.	N.-V.	V.-A.	N.-V.-A.	Average.
F. E. B.	1.67	6.17	6.17	2.92	4.27
E. T. J.	2.92	1.67	1.67	1.25	1.88
Average.	2.30	3.92	3.92	2.09	3.08

For the verbal series the errors in percentage are:

Subject.	A.-B.	A.-D.	B.-D.	A.-B.-D	Average.
J. J.	7.92	5.42	3.33	5.83	5.77
G. W. M.	8.92	12.92	12.92	14.54	12.29
F. E. B.	5.00	2.92	5.42	2.50	3.96
E. T. J.	4.58	6.17	5.00	1.45	4.38
Average.	6.61	6.86	6.67	6.08	6.60

Average of F. E. B. and E. T. J., 4.17.

For the pictorial series the errors in percentage are:

Subject.	A.-B.	A.-D.	B.-D.	A.-B.-D.	Average.
J. J.	1.25	3.75	0.63	0.67	1.56
G. W. M.	10.00	0.63	3.75	8.12	5.62
F. E. B.	0.63	1.25	1.87	1.25	1.25
E. T. J.	0.63	0.63	1.87	2.50	1.41
Average.	3.13	1.57	2.03	3.12	2.46

Average of F. E. B. and E. T. J., 1.33.

It appears that the percentage of error is smallest for the pictorial series, largest for the verbal series, and intermediate for the grammatical series. The individual difference that should be noted is the large number of errors of G. W. M., which is undoubtedly related to the shortness and mode of his reactions. The order of the four subjects regarding their liability to error is the same in the

verbal and pictorial series. It is also of interest to inquire whether the average time of these erroneous reactions is markedly different from the time of the correct ones. In the following table the average time of the erroneous reactions is given in percentage of the corresponding average correct reactions; and the results show, on the whole, no appreciable difference between the two. In the erroneous reactions there is probably a greater variation than in the correct ones:

	J. J.	G. W. M.	F. E. B.	E. T. J.	Average.
Grammatical Series.	. . . . .	. . . . .	94.8	119.5	107.1
Verbal Series	108.6	97.6	99.9	103.1	102.3
Pictorial Series.	97.2	105.3	107.0	97.2	101.7

On the Perception of Simultaneous Sense-Impressions.

With the assistance of George W. Moorehouse, Fellow in Psychology.

The error in indicating with which one of a series of visual impressions an auditory or other impression seems simultaneous, was first noted by Wundt. He studied it by having an index rotate in front of a graduated disc at a constant rate, or again by having it oscillate with a pendular movement, and noting to what stroke of the disc the hand seemed to be pointing when a bell sounded. The actual moment of the sound was determined by the observer by moving the pendulum slowly across the disc and the error in time was then calculated by mathematical formula. With this apparatus Wundt established that it takes many separate judgments before one is ready to make one's decision; that the error is very variable; that the error for almost all the rates of movement used is negative, i. e., the time at which the bell is said to have rung precedes the time of its actual ringing; that this error decreases as the speed increases, until it becomes positive; that the error increases in the accelerating portion of a pendular movement, and decreases in the portion of diminishing velocity; that the error with the constant motion disappears when one division[1] of the disc corresponded to $\frac{1}{15}$ of a second ($=28\sigma$) and the interval between successive sounds of the bell is one second; and finally the very important fact that the determination was considerably under the control of the will of the observer, and was influenced by the direction and nature of the attention.

Wundt also experimented with more than two simultaneous impressions, but his results on this point need not now be considered.

Tschisch (Wundt's Studies II. 603-634) has contributed an elaborate research, working with the same apparatus, but his main results are concerned with the determination of the error with several simultaneous impressions. Reference to his results will be made later on. Both Tschisch and Wundt connect with their results an elaborate theoretical interpretation.

[1] This division refers to the smallest portion of the divided circle taken into account in the subject's judgment; Wundt's apparatus as figured has a mark for every two degrees, but it is to be inferred that he judged only to the nearest ten degrees. The importance of this point is the subject of discussion below.

[For several reasons a reinvestigation of the fundamental factors of these interesting phenomena seemed desirable; the accepted interpretation of the error as the time needed for the reception and elaboration of the perception (*Complication einer Vorstellung*[1]) seemed questionable; the dependence of the error upon the apparatus as well as upon the mode of judgment seemed not to have been sufficiently regarded.] The phenomenon, when reduced to its simplest terms, may be thus described: There is a series of sense-impressions following one another in a recognizable order and the members of which are distinguished from one another both in time and by some other characteristic; a disparate and momentary sense-impression is interposed at some moment unknown to the subject, and he must determine with which one of the series of impressions the disparate impression seemed to coincide. In order to take note of small errors, it is necessary that the successive members of the series of sense-impressions be rapidly distinguished; and sight and hearing alone, therefore, are available for this purpose. It is true that we can distinguish both these and other sense-impressions by the artificial device of counting, but this process is too slow and absorbs too great a share of the attention to be here available. Sight is decidedly the preferable sense by reason of its superior power of taking in a large range of impressions at once; and in many ways the most convenient visual impressions are the divisions of a divided circle. The place of a given mark in the circle is readily determined when each fifth or tenth mark is differentiated from the others; the circle used by Wundt has a short stroke for each two degrees and a larger stroke for each ten degrees, and this division we have used in our experiments. A point travels along this visual scale, and for the interposed impression the stroke of a bell or an electric shock on the finger is most convenient. Our problem then is simply this: Where upon the divided circle was the moving point when the bell sounded or the shock was felt? The most important factor in this decision is obviously the accuracy with which the subject is required to decide; i. e., whether he is to determine the point when the bell sounded to the nearest ten, five, two or one degree; this is the one point that must be determined before the observations can proceed, and indeed must be considered in the preparation of the divided circle. And yet it is a surprising fact that this is the one point upon which former observers have been most reticent; one can only infer it, and that not too certainly from the apparatus used. Wundt, in his observations with an index revolving at a constant rate, judged to the nearest ten degrees; Tschisch apparently judged more accurately, and if by a division (Theilstrich) he means a division of Wundt's apparatus as figured, he judged to the nearest two degrees. The accuracy of the judgment with a given rate of movement is dependent upon the size of the divisions; after a certain velocity has been reached, we can no longer distinguish the several positions of the moving point. The larger the disc, the higher the speed as measured by the time of one revolution at which an interval of a given number, say two degrees, may be distinguished. Wundt's disc for constant movement was but slightly over six inches in diameter, and thus it is clear why he could judge only to the nearest ten degrees; in the apparatus of Wundt used by Tschisch, the disc is about 8½ inches in diameter.

[1] Tschisch has elaborated and Wundt has endorsed this interpretation as applied to more than two simultaneous impressions; and a table is given indicating the time of the several higher kinds of reception and fusion of perceptions (Tschisch p. 633; Wundt, 3rd Ed. II. p. 341).

To obtain a greater range of velocity of movement we used a much larger disc, 22 inches in diameter, divided by short strokes into two degrees, and by a longer one for every ten degrees. We judged not only to the nearest stroke, but also whether the point stood on or between two strokes when the bell sounded, i. e., we *judged to the nearest degree.* The distance on our disc between two strokes (two degrees) was 9.75 mm., or about $\frac{3}{8}$ of an inch; in Wundt's disc this distance was only 3.84 mm., about $\frac{1}{10}$ of an inch. Furthermore, to secure ease of reading, the markings were plain and bold, in black ink upon white card-board, and the index was blackened and tapered to a readily visible point.

In almost all previous determinations, the index moved with a pendular movement; while it is interesting to observe the effect of the change of velocity upon the error of judgment, it is certainly important to have determined the error for a constant rate as a standard of comparison; and to regard the rate at the base of a pendular oscillation as equivalent for this purpose to a constant rate is not free from objection. In all our observations, the index traveled over the disc at a constant rate.

This disc was glued to a board, 22 inches square, and the whole mounted in a vertical position; through a hole in the centre of the disc, the axle bearing the index projected and this index could be set in any position and then fastened by a thumb-screw. The mechanism by which the index was rotated was the clock-work of a clinostat. This apparatus was admirably adapted to our purpose and admitted of a great range of velocity. On the axle, behind the disc, was fastened a small wire, the end of which just dipped into a mercury drop, and thus in each revolution established an electric circuit. By this connection, a bell could be struck or an electric shock given to the finger, and by the setting of the index the point at which this occurred was charged. Moreover, a switch in each circuit enabled the observer to introduce the sound or the shock at any desired moment; this is important, as no judgment should be made until the clock-work has obtained its full and constant rate. The subject sat at a convenient distance before the disc, the latter concealing from his view all the mechanism by which the index was rotated as well as the bell, and called out the positions at which the sound or shock seemed to come.

A further point in the method of observation is of importance. The judgment in which the subject has any confidence is formed only after several observations, the point at which the impression was interposed shifting with each observation. There are two natural methods of recording the error; the one is to take the *average of all* the observations with a given setting of the interposed impression; the other is to ask the subject to decide upon *one judgment as the final* one, and to measure the error by this, recording, however, the several observations as well. After a trial of both, we adopted the latter plan as the better. The point at which the interposed impression really occurred was readily determined by slowly moving the index (by turning with the finger one of the fine wheels of the clock-work) until the bell sounded or the shock was felt. In order to have the sound or shock as brief as possible, the mercury cup was made in the form of a narrow slit, through which the point of the wire could be made to pass at any desired angle, and to prevent the sound of the bell from continuing after it was struck, the bell was loaded with drops of wax. In some cases we found it more convenient to use a spring wire instead of the mercury drop. The rate of the index was determined by timing to the nearest second three, five or ten revolutions, according to the

rate, and the result was expressed uniformly *in the σ=.001 of a second required for the point of the index to travel over one degree of the circumference.*

The results we have thus far accumulated are in every sense provisional; the number of observations is not adequate and they are offered at the present time simply because of the interest in the methods by which they have been obtained and their explicit disagreements with previous results.

We calculate from the tables of Tschisch, that with the index moving at such a rate that 1° was passed over in 3.07 σ, the error for sound was a negative one of 64.8 σ, for touch 64.8 σ, for electric shock 72.2 σ; with a faster rate of 1° in 2.41 σ, these errors were 44.1 σ, 44.1 σ and 39.9 σ; and with a still faster rate of 1° in 1.7 σ, these errors were 20.3 σ, 20.3 σ and 20.3 σ. In all cases these are the errors at the base of the pendular movement, when the acceleration is zero; and by a negative error is meant an error in judging the interposed impression as occurring in advance of its actual occurrence. The sound was that of a bell, the touch a tap of a hammer upon the frontal surface of the last joint of the forefinger and the third kind of stimulus was an electric shock, presumably upon the finger. Tschisch does not describe his manner of recording the judgments, whether he averaged all the observations or accepted a final judgment with each test; in what way he combined errors of opposite direction and the like. The chief characteristics of his result are the large size of the errors; the decrease of the error with an increase of speed and that, too, within small range; the constancy of this error with different kinds of interposed stimuli, and the negative character of the error throughout.

In our observations, the individual variation of the results is so very great that it seems somewhat strained to attach any importance to the general average. These variations are so great that in all the observations with any one rate of speed, observations with positive as well as negative errors occur. Furthermore, within the range of velocities studied by Tschisch, we can distinguish no constant tendencies at all, and within the very much larger range of velocities at our disposal nothing that could be dignified by the name of law appears. Taking all our observations together we find for the sound:

Rate,	1° in 2 to 4 σ.	1° in 4 to 6 σ.	1° in 6 to 9 σ.	1° in 9 σ or more.
Error,	—10.6 σ	+7.0 σ	+5.9 σ	+1.8 σ

These numbers are based upon 120 observations in all.

With the electric shock as the stimulus the results are:

Rate,	1°=2 to 4 σ.	1°=4 to 6 σ.	1°=6 to 8 σ.	1°=8 σ or more.
Error,	—15.8 σ	—3.5 σ	—6.4 σ	—45.1 σ

These results are based upon 130 observations in all.

These results, though entirely provisional and without much significance, owing to the great individual variations, yet are opposed to all the four main results of Tschisch's experiments. What this opposition means, it would be premature to say. But three points, further, need be noticed: (1) the relative constancy of the results when calculated without regard to their positive or negative characters; (2) the difference of individual observers in these observations; (3) the tendency of the several individual judgments in a single observation. With regard to (1) it is only necessary to indicate this fact: this error is not far from 30 σ for the sound, and from 40 σ for the electric shock, independently of rate. (2) We have tested a sufficient number of individuals to make great differences in the size and direction of the error, but not enough to describe them in quantitative terms. With regard to (3) we can only say that with

some individuals the index nearly always moves to a later point of the disc with successive sounds of the bell or shocks on the finger.

We did not confine our studies to this method of observation, but devised several others, to the description of which we may now turn.

We arranged a method by which a series of auditory impressions could be substituted for the visual ones. This arrangement requires the services of three persons; in one room there is the subject with his finger on the "shock key," listening to the reading of the observer; the observer, in reading, speaks into the mouth of the transmitter of a telephone, and at the other end of the telephone in another room sits the recorder; there is, further, an arrangement by which, either automatically or at the desire of the recorder, the shock may be given and the telephone circuit broken. The subject notes at what word he was listening when the shock came and the recorder records the last word heard before his telephone was cut out of the circuit. A simpler mode of observation consisted in connecting the shock circuit with the telephone circuit and noting between what words the slight sounds accompanying the making and breaking of the shock circuit came. We then measure the rate of speaking and calculate the error in σ. In reading from a book, the subject does not know what is coming; but in observing the movements of an index before a disc, the sequence of impressions is foreknown. We can secure the latter conditions for hearing by counting, or by speaking the alphabet. A further variation consists in having the subject himself count aloud or read aloud from a printed page; but this is not so serviceable as the other form of experiment. The result of all our experiments with the auditory series may thus be expressed: The error in indicating the place of a shock in an auditory series is less than one of the smallest units of time (the time needed to speak one word or one syllable) that we could take into account in the observations. The quickest sense-impressions that one can follow by ear is counting from one to ten repeatedly, but this can hardly be done more rapidly than seven per second; our result then simply shows that the error is rarely as large as one-seventh of a second $= 143\ \sigma$. It is further to be noted that the recorder's method of noting the error is not as objective as is to be desired. In noting the place of the two clicks in the auditory series, there is some though less opportunity for the same error of time-location as in placing the position of a shock in the same series. The experimentation is difficult, the results indicating that the ear cannot differentiate and locate the sequence of impressions with sufficient rapidity to permit of the detection of the error under consideration.

As a further contribution to the influence of the apparatus and mode of judgment upon these errors of location in time, we altered one of the most important conditions of all former experiments: instead of having the disc stand still and the index moved, the reverse was done. This was accomplished as follows: Upon a revolving drum was fastened a sheet of paper with various lines of letters, words and numbers written upon it with a type-writer; the drum was in a horizontal position, but to bring the letters in a vertical position and to have them pass across the field of vision from right to left (and thus be read from left to right) as well as to have but one line, or rather as much of one line as one could see, in sight at any one time, two mirrors were appropriately placed at the end of a shallow box, through which the subject read. A fine thread placed in front of one of these mirrors served as an index, the subject judging what letter or number was opposite the thread when

the bell sounded or his finger received the shock. As before, the drum moves at a constant rate, and a final judgment is recorded after several individual observations. We judged always to the nearest letter or number (there were 135 in a line), and in some instances we attempted judgments between or upon letters or spaces, that is, to the nearest half letter. It will be seen that a letter corresponds to 2½° or a half-letter to 1¼°. We used 12 different lines; lines I. and II. were continuous words from a story for children; line III., a series of detached monosyllabic words; line IV., miscellaneous numbers between 20 and 100; line V., the numbers in regular order from 20 on; line VI., the numbers advancing by 7 from 12 to 100, 11 to 100, 13 to 100; line VII., letters of the alphabet in chance order; line VIII., a line of verse; line IX., of prose; line X., the same line of prose, but in reversed order; line XI., a line of German; line XII., a scale of short uniform marks with every fifth mark heavier and numbered to correspond exactly with the divided circle with the rotating index.

Let us consider line XII. first, as that allows of most direct comparison with former results. With this method of judging a much slower rate is necessary; the circle is much smaller, about 4 inches in diameter; it is more difficult to read the lines while in motion, and a smaller portion of the circle is visible at any one time.

With the bell our results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 38 to 42 σ.	75 observations.
Error,	—27.6 σ	—20.7 σ	—28.8 σ	

With the electric shock, the results are :

Rate,	1° in 20 to 24 σ.	1° in 24 to 28 σ.	1° in 32 to 50 σ.	60 observations.
Error,	—50.7 σ	—23.2 σ	—54.4 σ	

The great variability of the results is again a striking factor, though they are almost uniformly negative. There is no definite connection suggested between rate and error, and the error is different with the bell and with the shock.

The other eleven lines were arranged to furnish material for the study of the effect of the different kinds of visual series upon the error. Most of the observations were, therefore, made with one rate of speed, about 1° in 20 σ. If we divide the lines into those containing continuous words, I., II., VIII., IX., XI., those containing detached words or letters, III., VII., X., and those containing numbers, we find as the general average error of the first set $+2.2\sigma$, of the second $+7.0\ \sigma$, of the third, $0.0\ \sigma$. There is no difference of note between the results of bell and shock. The small error and great variability of the results are again the marked characteristics; the averages have thus little significance. But one further result is worthy of notice. If, instead of recording simply the final judgment, we record all the individual answers and enter their average as a result, then the error seems to be larger; we can make such a comparison for the "shock-stimuli," though the two sets of results were not taken at the same rate (the "average" judgments being at about half as rapid a rate as the "final" ones). In the first case the average error is 4.1 σ and in the second case 35.7 σ. Attention is again directed to the provisional character of the results throughout, and to the fact that our main objects are the analysis of the factors involved in these observations, the indication of the dependence of the results upon method, apparatus and mode of judgment, and the recording of the absence of agreement of our provisional results with those obtained by other observers.

THE PSYCHO-PHYSIC SERIES APPLIED TO LIFTED WEIGHTS.

With the assistance of WALTER D. BROWN.

The method of the psycho-physic series presents to the subject a range of sensation intensities—usually successively one at a time—and requires him to assign each sense-impression to one of a given number of divisions or magnitudes. By this method we may compare the assigned magnitudes of the stars with their photometric intensities and determine whether the subjectively equal different magnitudes (arithmetical series) correspond to a similar objective series or—as the psycho-physic law demands—to a series of equal ratios (geometrical series). In former contributions (Vol. I. pp. 112-127, Vol. III. pp. 44-79, Vol. IV. pp. 213-219) the close correspondence of estimated star-magnitudes with the law was shown, and the method applied with varying results to sensations of visual and tactual expression, to the time-sense and to the motor-sense. A comparison of all the results suggested the generalization that the law probably holds of sensations that are appreciated *en masse*, without conceiving them as divided into units, on a general unanalyzed impression.

Our present study applies this method to the sensations obtained in lifting weights in the palm of the hand, and this includes the sense of muscular contraction as well as the pressure-sense of the palm. Sixty weights were prepared, the lightest weighing 12 grms. and the heaviest 795 grms.; the intermediate weights corresponded to the average of an arithmetical and geometrical series inserted between these limits; in this way the selection of weights, while from the subject's point of view essentially a matter of chance, favored one result no more than the other. A set consisted of 60 observations, each weight being assigned once to a magnitude. The subject, without distinctly seeing the weight, lifted it up and down in his right hand and assigned it according to his sensations, to one of six magnitudes or classes. The lightest weights were grouped as Class I., the heaviest VI. The order of the weights was determined by chance. The weights were made by packing cylindrical boxes $3\frac{1}{2}''$ high and $1\frac{5}{16}''$ in diameter with leaden discs cut to fit the inside of the box and supplemented by felt discs, cotton and shot; the weight was equally distributed throughout the box and were all alike in appearance, being marked by a letter on the bottom. Two sets (120 judgments) were taken upon seven subjects and four sets (240 judgments) upon three subjects. The average weight in grammes and the results of all the weights assigned to each of the six magnitudes or compartments I., II., III., IV., V. and VI. by each of the new subjects is given in the following table, the last line giving the average of all:

Subject	No.	I	No.	II	No.	III	No.	IV	No.	V	No.	VI	No.
J. H. T.	120	39.4	23	103.2	23	175.4	14	249.2	14	370.1	18	619.4	28
F. S.	120	43.9	23	92.3	23	178.9	16	257.2	14	400.8	21	640.4	23
J. H. D.	120	25.8	13	71.2	17	146.0	28	286.0	22	438.5	20	664.3	20
F. E. B.	120	27.3	14	74.6	19	131.6	18	240.4	24	391.8	21	640.2	24
E. P. S.	120	23.6	19	91.1	20	181.5	28	323.4	17	461.7	19	696.2	17
C. M. R.	120	34.5	20	97.6	23	170.7	18	285.2	23	433.9	15	660.0	21
E. T. J.	120	40.5	26	127.8	29	239.3	20	369.2	16	526.2	14	676.1	15
G. W. M.	240	27.0	30	58.0	41	148.2	39	232.6	36	370.2	45	624.3	49
W. D. B.	240	29.5	30	75.8	34	145.1	46	255.8	44	408.9	40	643.8	46
J. J.	240	28.2	30	76.2	39	151.4	47	266.1	45	422.6	28	620.1	51
Average	1560	32.0	228	86.8	268	166.8	274	276.5	255	422.5	241	648.5	294

In the next table are given for each subject, in the upper line, the successive *differences*; in the lower line, the successive *ratios* between the average weights of neighboring magnitudes; in the last three columns are found the averages of these differences and of these ratios, the average deviation of the several differences and of the ratios from their mean (expressed in percentage), and the ratios of these percentages of deviation to one another. In the lowest lines of the table, similar results are given for the general average of all.

Subject	I-II	II-III	III-IV	IV-V	V-VI	Average	Average Deviation	Ratio
J. H. T.	63.8	72.2	73.8	120.9	249.3	116.0	47.7%	1: 2.51
	2.62	1.70	1.42	1.49	1.68	1.79	19.0%	
F. S.	48.4	86.6	78.3	143.6	239.6	119.3	48.4%	1: 3.56
	2.10	1.94	1.44	1.56	1.60	1.73	13.6%	
J. H. D.	45.4	74.8	140.0	152.5	225.8	127.7	42.4%	1: 2.37
	2.76	2.05	1.96	1.53	1.52	1.96	17.9%	
F. E. B.	47.3	57.0	108.8	151.4	248.4	122.5	50.4%	1: 2.77
	2.73	1.77	1.82	1.63	1.63	1.97	18.2%	
E. P. S.	67.5	90.4	141.9	138.3	234.5	134.5	35.7%	1: 1.89
	3.90	1.99	1.77	1.43	1.51	2.12	18.9%	
C. M. R.	63.1	73.1	114.5	148.7	226.1	125.1	39.8%	1: 1.93
	2.82	1.75	1.67	1.52	1.52	1.85	20.6%	
E. T. J.	87.3	111.5	129.9	157.0	149.9	127.1	17.4%	1: 0.62
	3.15	1.87	1.54	1.43	1.28	1.85	28.3%	
G. W. M.	31.0	90.2	84.4	137.6	254.1	119.5	53.8%	1: 2.97
	2.15	2.55	1.60	1.59	1.69	1.91	18.1%	
W. D. B.	46.3	69.3	110.7	153.1	234.9	122.9	46.3%	1: 2.86
	2.57	1.91	1.66	1.60	1.57	1.86	16.2%	
J. J.	48.0	75.2	114.7	156.5	197.5	118.3	39.7%	1: 2.17
	2.66	1.99	1.76	1.59	1.47	1.91	18.3%	
Average	54.8	80.0	109.7	146.0	226.6	123.3	40.6%	1: 2.04
	2.71	1.92	1.65	1.53	1.53	1.87	19.9%	

The general result is indicated in the last entry in the column of ratios; *the approximation to a geometrical series is, on the average, twice as close as to an arithmetical series; to this extent the psychophysic law is followed.*

Passing to the individual results, it is seen that all the individuals, with one exception (E. T. J.[1]), favor the geometric series, and of these nine, six approximate it more closely than the general result. If we omit the one divergent record, the general deviation from an arithmetical series becomes 43.4%; from a geometrical series, 17.8%, and their ratio as 1: 2.44. We have, then, a coarsely approximate geometric series, but one which presumably is fairly constant in different individuals.

In the application of the method of the psychophysic series to the time-sense, it was found that the first set of those subjects upon whom more than one set was taken, conformed much more closely to a geometrical series than did the following ones. The same is true of only one of the three subjects who contributed two sets to the present study. For W. D. B. the ratio of approximation to a geometrical series to that of an arithmetical series, as in his first set, 1: 5.27; in his second set, 1: 1.60; in the two combined, 1: 2.86; for G. W. M. these ratios are 1: 2.83; 1: 2.42; and 1: 2.97; for J. J., 1: 1.63; 1: 1.90; and 1: 2.17. This would indicate that practice has less tendency to change the method of judging lifted weights than of time-intervals; in the latter case the approximation to a geometric series is much closer than in the former.

It will be observed that the deviation from a geometric series proceeds, not in a hap-hazard way, but exhibits a fairly definite and constant tendency. The ratio between the average weights of neighboring magnitudes is not a constant, but decreases by smaller and smaller steps, and thus approaches a constant. The unusually high ratio between the classes or magnitudes, I.-II. is a common characteristic of such results (see this Journal, Vol. I. p. 123, Vol. IV. p. 216) and is in large measure accounted for by the fact that the number and average weight of observations falling in Class I. are affected by there being no class smaller than I., to which doubtful judgments might be assigned. The decline of the ratios was in the case of the star-magnitudes expressed by an empirical formula, making the ratio a constant multiplied by a constant times the excess of the magnitude, above a given magnitude. The ratio is expressed, not by a straight line, as it would be if it were a constant, but by a line inclined to the horizontal at a slight angle. A similar inclination, though not a constant one, is suggested by the present results.

The suggestions offered in former applications of this method are entirely corroborated by the present study; the tendency to have equal ratios of objective stimuli correspond to equal sensation-differences is strong and natural in such types of sensation as are estimated grossly and from an impressionist point of view, without reducing them to units or conceiving them as thus reduced. We are quite likely to gauge weights by an unanalyzed feeling of effort, which we do not tend to reduce to pounds and ounces, and this is the natural basis of the psychophysic law. We reserve for a future contribution the general discussion of all the results thus far obtained in the application of the method of the psychophysic series to various types of sensation.

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[1] In this case there are reasons for believing that the subject took a very artificial view of the problem before him, and more or less consciously favored the arithmetical series.

Note Upon Other Researches.

To complete the account of the studies of the year, mention may be made of a few studies, as yet incomplete, or to be published elsewhere. In collaboration with Mr. Geo. W. Moorehouse, a new æsthesiometer has been devised, which differs in essential points from those now in use. It permits of testing the sensibility of the skin with a variable pressure upon the points of the skin tested; the motion by which the points are applied is constant, regular and simple. Furthermore, a series of attachments is to be constructed by which the same apparatus may be used for exploiting the various types of tactile sensibility, for the pressure sense, and for the temperature sense; the apparatus will thus test all the chief sensibilities of the skin. The construction is not elaborate and the cost will be moderate.

Two researches of a statistical nature have been undertaken and are nearing completion. One is a study of the dreams of the deaf, with a view of determining the effect of the age of becoming deaf upon the future retention of "dream hearing," and of recording many other peculiarities of the dreams of this class. Mr. E. T. Johnson has had charge of the tabulation of this interesting but troublesome material. The other is a study of association and community of thoughts; the main point being to determine in what degree different persons are apt to think of the same association when starting from a common point, and then following their own line of association. A word is given to a class of students, and at the same time each member of the class writes the first five words suggested by the original word. The proportion of similarity of association in all the first words written, in all the second, etc., as well as in the sum total of all the words is the chief point to be studied; and the main result is the regular decrease of community of association as the words are removed from the original word. The first words suggested to different persons by a given word are more apt to be the same than the second, the second more so than the third, and so on.

STATISTICS OF "UNCONSCIOUS CEREBRATION."

By Charles M. Child, Wesleyan University.

The present article is an attempt to give in a statistical form the results obtained from a set of questions on "Unconscious Cerebration." These questions were first issued by Mr. Francis Speir, Jr., of South Orange, N. J., and a part of the results of his investigation were published in the *Popular Science Monthly*, Vol. 32, p. 657, under the title, "*The Antechamber of Consciousness.*" In order to the continuity of the inquiry it seemed best to re-issue the same set of questions, and these Mr. Speir very kindly furnished, together with the answers which had been returned to him. These answers were mostly from students of various colleges, and from persons in professional life. To these were added about a hundred more, all of college students, making the whole number of answers two hundred. These latter were collected by Professor A. C. Armstrong, Jr., of Wesleyan University, under whose direction the present investigation has been carried out. From these answers the statistics have been compiled, first, in general, with no regard to sex or age or other conditions; then the sexes were separated and the percentages for each were obtained; and third, the percentages were computed for the different ages.

In the two hundred papers there are one hundred and fifty-one from men and forty-nine from women. As regards age, the greater portion of the persons answering are between twenty and thirty years, and more of these are under twenty-five years than above. As the papers naturally fell into several divisions according to age, it seemed advisable to separate them as follows: first, those under twenty-five years; second, those between twenty-five and thirty years; and third, those over thirty years of age. The number of persons in each division is as follows: ninety are under twenty-five, thirty-two are between twenty-five and thirty, and forty-one are over thirty, besides which there are thirty-seven who do not give their ages.

Before giving the statistics a few words of explanation may be necessary. Each question is given separately, and following it are the percentages, together with any examples or remarks. A part of the examples quoted here are from the papers furnished by Mr. Speir and a few of them are given in his article; the others are from the papers collected by Professor Armstrong. In explanation of the figures it may be said here that in the tables the horizontal series headed "whole number answering," those answering "no" and "indefinite" have been computed only in the general division, and, when it is not otherwise stated, are percentages of the whole number of answers returned, i. e., two hundred. The figures in the different divisions headed "men," "women," etc., are, unless it is otherwise stated, percentages of the number of persons in each division. All the percentages are given as whole numbers, fractions of one per cent. being discarded. This sometimes causes a slight apparent discrepancy, as, for example, that noted below under the

first question. With this explanation and the notes given with each table, the figures will doubtless be clear. Only a part of the questions are given in tabular form, as it was unnecessary to give all the figures in every case. In the questions as given below the original order has been somewhat changed, and some portions, as well as some entire questions, which elicited answers of no essential value to the subject, have been omitted. The questions omitted are those numbered I., V. and XI. in Mr. Speir's original list. In other respects our list is identical with the original.

QUESTION I. 1. When you are unable to recall the name of something wanted and you say, "Never mind, it will occur to me," are you conscious of any effort of searching after it?

2. When you are, do you feel some trouble or weight in your effort?

3*a*. Does the idea ever seem to have come back spontaneously without being suggested by any perceived association of ideas?

b. Does such recovery of the lost idea ever come during sleep?

c. Does such recovery come after sleep?

d. Please give examples from your own experience, illustrating fully.

	1	2	3*a*	*b*	*c*
Whole number answering.	93	81	92	86	83
Those answering no.	21	12	11	68	31
Indefinite.	0	0	0	4	6
	Yes.	Yes.	Yes.	Yes.	Yes.
General.	72	68	81	17	57
Men.	72	68	77	18	60
Women.	73	67	84	15	54
Under 25 years.	72	72	79	10	51
Between 25 and 30 years.	78	66	84	33	56
Above 30 years.	66	61	71	21	65

In the first two columns there is an apparent discrepancy due to disregarding fractions of one per cent. In the first column the general percentage is 72, that of the men 72, while that of the women is not, as the general percentage would seem to indicate, 72, but 73. The same variation is seen in the second column. The percentages in the vertical columns under *b* and *c*, with the exception of the first three in each column, are percentages of the number of those in each division who answer 3*a* affirmatively.

In the answers to 1 there is little variation except in the last two divisions. Those between twenty-five and thirty show a distinct rise, and those above thirty a fall in their percentage. Under 2

those under twenty-five are above the general percentage, those between twenty-five and thirty somewhat below, and those above thirty still further below. In 3*a* the women show a higher percentage than the men, and here there is again the distinct rise between twenty-five and thirty, while those above thirty are considerably below those under twenty-five. Under *b* a somewhat larger percentage of men than of women answer affirmatively; the percentage of those between twenty-five and thirty is about double the general percentage, while that of those under twenty-five is less than the general percentage. In *c* also the percentage of affirmative answers is larger among the men than among the women. Here there is an increase in the percentage of affirmative answers with increase of age.

A few examples, which are among those given in the papers under the general subject of the spontaneous recovery of ideas, may be interesting and are given below.

1. "This morning I endeavored to recall the name of the characters I had read of in one of Scott's novels the night before. I could remember but one, and then only with much effort. During the morning I was unable to recall any other character by name, although constantly endeavoring to do so. After teaching a Sunday-school class, I walked home in the afternoon with my mother, and, without any effort, gave not only the names of the principal characters but many of the unimportant. I had not thought of the work for a number of hours."

2. "I was trying to think of the name of a book, and gave it up. About half an hour after, I was talking of something else when, all of a sudden, I blurted out the name without any conscious volition on my part, or without thinking anything about the book at all."

3. "I have tried to think of the name of a person without success in the evening, and the next morning have had it come to me without any connecting ideas at all, but it just seemed to 'pop' into my mind."

4. "I was telling my sister of a young lady, but I could not remember her name, though I thought I knew it. At last I had to give it up, and after a while forgot all about it, though I could not at first force myself to think entirely of other things. For a time I was dimly conscious of trying to remember. The next morning the name suddenly flashed across my mind, apparently without being suggested by anything else."

Many other examples are given, and a number state that the phenomenon is of very frequent occurrence. Several of the answers give empirical schemes for recalling the lost ideas, such as running through the letters of the alphabet, or working up from connected ideas to the one required.

QUESTION II. 1. Can you wake precisely at a given hour determined upon before going to sleep, without waking up many times before the appointed time?

2. If you can, (*a*) is this habitual, or do you often fail?

b. Are you conscious before waking of any feeling (describe it)?

c. Do you come directly from oblivion into consciousness?

	1	2a		b	c	
	Yes.	Seldom fail.	Often fail.	Yes.	Directly.	Gradually.
General	59	69	25	30	64	16
Men	62	69	28	33	56	16
Women	51	68	12	20	80	16
Under 25	68	66	33	33	62	15
Between 25-30	47	73	20	33	60	13
Above 30	61	68	12	16	64	20

Those who answer 1 are ninety-one per cent. of the whole number; those answering 1 in the negative, thirty-one per cent.; those answering indefinitely, one per cent. The percentages in the first vertical column are computed on the whole number in each division. The percentages in the other five columns are computed on the number of those in each division who answer 1 in the affirmative.

As regards the general percentages, the table shows that fifty-nine per cent. of those sending in papers possess the power of waking at a given time without being disturbed before. About two-thirds of these seldom or never fail in their attempt. Only about a third of them are conscious of any feeling as they wake, and about two-thirds wake directly. In the other division the important points appear to be as follows: A smaller percentage of women than of men possess the power of waking at a given time. Those of both sexes between twenty-five and thirty years are also far below the general percentage in the possession of this power. Those under twenty-five are above and those over thirty are about equal to the general percentage. There is a distinct decrease with increasing age in the percentage of those who often fail in their attempt to wake at a given time. A smaller percentage of women than of men wake with any special feeling, and those above thirty only about half as often as those below. A very large percentage of women wake directly, while men are rather below the general percentage. A larger proportion of persons above thirty than of those below wake gradually. The feeling of which some are conscious on waking is variously described, but is in nearly all cases a troubled feeling, as some describe it, "a feeling that I must wake," "that something must be done," "that it is time to get up," etc. In answering *c* some of those who say they wake directly have a very distinct feeling at the time of waking, so that *b* and *c* are not mutually exclusive.

A few of the examples given are quoted.

1. "Yes, at an early or unusual hour, by repeating the time to myself once or twice before going to sleep. I seldom wake before the hour determined upon and never fail then."

2. "I was intrusted by the attending physician with the administering of medicine to my wife, who was very dangerously ill. It was of the greatest importance that a certain medicine should be given every two hours as exactly as possible. I am an extraordinarily sound sleeper, but for six weeks I woke up every two hours

methodically, and never missed giving the medicine. I always came directly from oblivion into consciousness. I was as exact and methodical during the first few nights as at the last."

3. "I have never overslept when my mind was charged before retiring."

4. "I can always wake at any hour I desire, usually a few minutes before."

5. "Always can wake just five minutes before the hour at which I set the alarm."

6. "I recall one instance more remarkable than any other in my own case. I had been broken of my rest every night for a week or ten days, and one evening retired at about nine o'clock, giving directions to be called at twelve o'clock. I fell asleep at once, and slept till twelve without waking. At that time something seemed to tell me it was twelve o'clock. I seemed to come from perfect oblivion to perfect consciousness. I rose and dressed just as the clock struck twelve. I was under the impression that some one had called me, and was surprised to learn that no one had spoken to me.

QUESTION III. 1. When perplexed at your progress in any work (mathematical, professional, literary, chess, puzzles, etc.), have you ever left it unfinished and turned your attention to other things, and after some time, on voluntarily returning to it, have found yourself able at once to satisfactorily master it?

2. If you have, please give instances.

The answers to the first part are as follows: Ninety per cent. answer the question, seventy-seven per cent. affirmatively and twelve per cent. negatively. Of the men seventy-seven per cent. answer affirmatively, while the percentage in the case of the women is eighty. Those under twenty-five show a percentage of eighty-four answering affirmatively, those between twenty-five and thirty, eighty-one, and those above thirty only seventy-three, a distinct decrease with increase of age. About sixty-four per cent. of those answering are able to give examples of such an experience, while many others say they are sure they have observed something similar, but cannot recall instances.

A large number of the examples given relate to mathematics, a considerable number to the translation of foreign languages, and some to other work, such as essays, puzzles, etc. Some of the examples given will serve as illustrations:

1. "Often while playing chess or working an example I have not succeeded well. On returning after having left it for a while, what was difficult before seemed now very easy."

2. "In working mathematical examples in the evening I sometimes 'get stuck.' I leave it over night and take it up in the morning, and I often get the answer immediately. So in translation I find passages that I cannot get out. I study on them for a while and then leave them for several hours, or better sometimes days, and I can get them clearly."

3. "In writing music I often get to a stumbling-block, and try vainly to search for a chord or bar of music, but cannot find the thing I want. When it gets me very excited I leave it and go for a walk, and on coming back to work, I will most likely be able to write it out at once, seemingly without any work on my part; it is all ready for me to put down. I have frequently had the experience."

4. "I have come across a sentence that was particularly difficult in some Latin book I was reading, and have been unable to translate it. I have then turned my attention to abstruse problems in mathematics, and worked for some time. On returning to the

Latin I have often found it quite simple, and have sometimes translated it at sight."

QUESTION IV. 1. During sleep have you ever pursued a logical, connected train of thought, upon some topic or problem, in which you have reached some conclusion, and the steps and conclusion of which you have remembered on awakening?

2. During a half sleep?

3. If you have, how does the result appear when measured by your normal standard of day-time mental activity, with regard to accuracy, etc.?

4. Please give examples illustrating your meaning in full.

The general answers are as follows: Ninety-three per cent. answer the first section of the question, fifty-nine per cent. have had or recall no such experience, while thirty-one per cent answer affirmatively. The second section is answered by eighty-two per cent., fifty-four per cent. in the negative, and twenty-four per cent. in the affirmative. Seventeen per cent. state that the results appear about as good or better than those reached in waking life, while eighteen per cent. reach conclusions which are far less accurate or absurd.

In the other divisions there is little variation, so it is unnecessary to give all the figures. There are, however, one or two points worthy of note. Only twelve per cent. of the women remember having any logical or connected train of thought in a half sleep, but the general percentage is twice as large. The low percentage of the women here may be connected with the fact that a very large percentage of women wake directly, as was shown in the fourth section of the second question. On the other hand, twenty-four percent. of the women reach results which are at least fairly accurate, this being somewhat above the general percentage, which is seventeen. The percentages of the different ages do not vary far nor with any regularity from the general percentages, and are not given.

Examples under this question are given by forty per cent. of those who have had an experience in sleep or in a half sleep; the following are quoted as showing the degree of accuracy sometimes attained:

1. "I have played a game of chess in my sleep. The game seemed in my sleep to be entirely completed. In the morning I remembered all but one or two plays, and when I played the game over in the morning it seemed consistent. I do not think that I had ever played that game (i. e., a game with those identical moves) before, and I could not play it now. I had been playing a great deal at the time, though, and of course had been thinking of chess when I went to bed."

2. "I have been puzzled by a problem in algebra which I found it impossible to solve, and let it rest over night, and while asleep have thought out each step and remembered it, and in the morning on trying the problem again, solved it without difficulty."

3. "Being greatly troubled over a problem in algebra just before going to sleep, and leaving the problem half finished, I dreamed the rest of the solution and obtained the correct result. On awaking, I remembered it, and it was correct."

4. "In my senior year at college I had an essay to write that troubled me unusually. After trying to decide upon the subject until quite late, I fell asleep and dreamed not only of the subject and analysis, but of all the details. The next morning I wrote out what I had dreamed, and found it far more satisfactory than anything I had ever done in the same line before."

"Two years before I had exactly the same experience about an equation in algebra which I worked out correctly in sleep."

5. "Have worked out many algebraic or geometrical problems during sleep. Have, when some years ago in Worcester Academy, scanned some fifty or seventy-five lines of Virgil not yet translated, except ten or fifteen lines; felt tired, went to bed, in sleep accurately translated all of it, and remembered it on waking."

6 "One evening had been working late on a hard geometry problem, and had failed to solve it. The next morning on awaking I remembered having dreamed of doing it and of obtaining the correct solution. I immediately went over the solution as I had in my dream, and found my reasoning all correct. If I had not thought of my dream immediately on waking up should probably have forgotten my solution, for it was even then hard to recall it."

7. "I had earnestly been trying to make a trial balance and had at last left off working, the summary of the Dr. and Cr. sides of the account showing a difference of £2 10s. 0d., the Dr. side being so much smaller. The error I had not found on Saturday night when I left the counting-house. On this same Saturday night I retired, feeling nervous and angry with myself. Some time in the night I dreamed thus: I was seated at my desk in the counting-house and in a good light; everything was orderly and natural, the ledger lying open before me. I was looking over the balance of the accounts and comparing them with the sums in the trial balance sheet. Soon I came to a small account having a debit balance of £2 10s. 0d. I looked at it, called myself sundry uncomplimentary names, spoke to myself in a deprecating manner of my own eyes, and at last put the £2 10s. 0d. to its proper side of the trial balance sheet, shut up and went home. Here the dream abruptly ended. I arose at the usual Sunday time, dressed carefully, breakfasted, went to call on some young lady friends, and to go to church especially with one of them. Suddenly the dream flashed on my memory. I went for the keys, opened the office, also the safe, got the ledger, turned to the folio my dream indicated. There was the account whose balance was the sum wanted, which I had omitted to put in the balance-sheet where it was now put, and my year's posting proved correct."

QUESTION V. 1. Have you ever been conscious of having discovered something new, e. g., an invention, a literary or poetical creation, or a mathematical solution, etc.?

2. If yes, then has this flashed into consciousness in the form of a clear conception?

3. How many instances can you give?

Seventy-two per cent. answer the first section of the question, forty per cent. negatively, and thirty-two per cent. affirmatively. Of those answering affirmatively, seventy-one per cent. have the idea flash into consciousness in a clear and distinct form. The percentages of both sexes are like the general percentages. Twenty-eight per cent. of those under twenty-five years of age think they have made such a discovery, thirty-two per cent. of those between twenty-five and thirty, and thirty-seven per cent. of those above thirty. Sixty-eight per cent. of those under twenty-five who have made such a discovery state that it came as a clear conception, seventy-five per cent. of those between twenty-five and thirty so state, and sixty per cent. of those above thirty.

These answers show, as might be expected, an increase in the number of such discoveries with increase of age. The percentages of those who answer the second section show an increase between twenty-five and thirty, while above thirty the percentage falls

below either of the others. Perhaps this may be due to the greater ability of the adult's fully developed mind to seize upon a hint as a basis, and work out from it the new idea.

A few of the examples given are quoted.

1. "I can instance as frequent the smallest kind of literary creation, forms of verbal expression, what one may call an apt phrase coming to my mind suddenly, uncalled for, as if uttered by some one else, of no use to me at the time or perhaps ever."

2. "Many instances of mathematical or psychological problems have suddenly flashed across my mind when on a totally different subject; sometimes very distinct and sometimes indistinct, which I afterwards developed into distinctness."

3. "Have often awaked with part of an essay all ready, with a letter wholly prepared; once or twice with a few stanzas composed on subjects that I had endeavored to treat in rhyme, once or twice also on subjects that I had not attempted or thought to write upon in verse."

4. "In one case I wrote a long piece of a rather satirical character, in easy rhythm, as fast as I could set down the words, and it needed little or no revision. Usually I am dissatisfied with my first copies."

QUESTION VI. 1. On seeing a sight (e. g., on visiting a strange place) or on hearing a sound (e. g., yourself or another making a remark), have you ever felt that you had under previous identical circumstances experienced the same before?

2. If you have, then give instances.

3. Describe any general feeling that accompanies this flash of half intelligence.

	1 Yes	3 Describe some feeling.
General	59	67
Men	56	65
Women	71	71
Under 25	69	68
Between 25 and 30	41	92
Above 30	63	50

Eighty-eight per cent answer the first part of the question, twenty-seven per cent answering negatively, and fifty-nine per cent affirmatively. The percentages in the second vertical column are computed on the number of those in each division who answer 1 affirmatively.

As regards the first part of the question, women show a larger percentage in the affirmative than men. Those under twenty-five show a larger percentage than any other age, and one which is above the general percentage, while those between twenty-five and thirty show a much smaller percentage than any other age. In the third section the women again show a higher percentage than the men; those of both sexes between twenty-five and thirty give a very high percentage, and those above thirty are much below the general percentage. The answers to this section vary greatly as regards the nature of the accompanying feeling. Many call it a feeling of annoyance, perplexity or surprise. Some say they have almost a feeling of awe, and one or two call it uncanny.

Sixty-three per cent. of those who recall such an experience are able to give examples. The greater part of these relate to sights or sounds, i. e., remarks, etc., heard or scenes visited, pictures seen and the like. Two persons, however, state that they have had this experience in connection with the sense of smell, but do not give definite examples. A few of the instances related will serve to show their nature:

1. "I have purchased a Chinese umbrella-stand which I know I never possessed before, nor can I recall ever having seen one like it. Yet it is impossible for me to see it without feeling that I have previously owned and used it."

2. "When driving over a new road in a part of the country where I had never been before, and of which I had never seen pictures, it seemed as though I had been over it before under perfectly identical circumstances."

3. "On meeting strange people, a word or look will convince me that I have seen the same thing done by the same person in similar circumstances."

4. "Sometimes I find places which seem to be places I have seen before, and I often find them to be places I have dreamed about."

QUESTION VII. 1. Do you dream?

2. If you do, then (*a*) give any characteristic peculiar to dreaming on the right side, left side and back.

b. Are you conscious of a moral sense during sleep?

	1	2*a*	*b*
Whole number answering	96	79	78
Those answering no	2	26	24
Indefinite	0	22	6
	Yes	More or worse on back	Yes
General	94	31	48
Men	93	28	46
Women	94	39	54
Under 25	98	25	47
Between 25 and 30	87	50	57
Above 30	85	29	43

All the percentages in the second and third vertical columns of figures are computed on the number of those answering the first section of the question affirmatively. The first four in each column are computed on the number in the general division answering 1 affirmatively, and the other five on the number answering in each division respectively, the first section affirmatively.

The sexes show no appreciable variation from the general percentage in the first section. The second section shows that the dreams of women are more affected by position than those of men, and the third section shows that a larger percentage of women than of men are conscious of a moral sense when dreaming. In the answers from those of different ages, there is a continuous decrease with increasing age in the number of those who dream. Those under twenty-five are least affected by position. Those over thirty show a somewhat higher percentage, and those between twenty-five and thirty give a percentage nearly double that of the other two divisions. Again, in the answers to the third section of the questions, there are more between twenty-five and thirty who are conscious of a moral sense than in any other division, while the percentage of those over thirty years of age is the lowest of all.

Besides these answers, two persons state that their dreams are unpleasant when they sleep on the left side as well as when on the back, and one person says he dreams less when lying on the back than in any other position. Two others answer that any unusual or cramped position seems to produce disagreeable dreams.

QUESTION VIII. 1. Do you talk in your sleep?

2. If you do, answer from your family report:

a. Are you able (if accosted when so talking) to answer intelligently questions put to you? If yes, then (*b*) do you answer any questions, or only questions on the subject that you are talking about?

	1 Yes	2*a* Yes	*b* Any	*b* Only on subject
General	40	37	27	43
Men	41	32	25	55
Women	37	56	30	20
Under 25	58	42		
Between 25 and 30	19	17		
Above 30	19	37		

Eighty-nine per cent. of the whole number answer the first section, forty-eight per cent. negatively, and forty per cent. affirmatively. The percentages in the first column are computed on the number of persons in each division; those in the column headed 2*a* on the number in each division who answer 1 affirmatively, and the two columns under *b* on the number in each division who answer 2*a* affirmatively.

The figures show that a rather larger proportion of men than of women talk in their sleep, while the percentage of women who answer questions when asleep is much larger than that of the men. About twice as many men answer only on the subject concerning which they are talking, as on any subject, but with the women the percentage of those who answer on any subject exceeds the other. The percentage of those who talk in their sleep is much higher among those under twenty-five than among those above. In the ability to answer questions, those under twenty-five stand highest and those between twenty-five and thirty the lowest. The figures under *b* are not given for the different ages, as they show no special variation from the general percentage.

The existence of phenomena like the above has long been admitted by psychologists. Sir William Hamilton, to go no further back, in Lecture XVIII. of his *Lectures on Metaphysics*, collected and discussed a number of examples of them; and Carpenter, in his *Mental Physiology*, also devotes a chapter to the subject. These examples and discussions, unscientific though some of them were, led the way to further investigation, and are abundantly confirmed by later works on the subject. That there is a field of mental or cerebral activity which lies without the limits of consciousness, is regarded as an established fact. This paper has attempted to show the relative frequency of the phenomena involved in this "unconscious cerebration," and the results reached give an experimental verification of the psychological principles which others have maintained. The general sources of error in statistics of this nature are well known, and of course enter here to a greater or less extent. Some who answer evidently do not understand the questions and some are in doubt as to their answers. A number answer who are unable to give definite examples, but "think" they have noted such an experience. But there are, on the other hand, reasons which are sufficient to counterbalance these sources of error, and to give the results undoubted validity. First, there is in

most cases a large percentage of those who state that they have certainly experienced the phenomena under discussion, and a large proportion of these are able to give definite examples in confirmation of their statement. Again, there is often a considerable number who state definitely that they have had no such experience as the question indicates, and this, in a negative way, still further confirms the results.

A difficulty of considerable importance in the collection of statistics, is that of selecting a body of persons who will give average results. This difficulty has been felt to some degree in the present inquiry. The answers are from persons more or less accustomed to mental work and self-observation; nearly one-half of those who answer are under twenty-five years of age, while thirty-seven do not give their ages at all; and there are three times as many men as women. But the error arising from these facts cannot be very large, and may probably be disregarded, as the persons from whom these statistics come are in other respects a body well fitted for such purposes. They are largely college students and, moreover, not special individuals among the students, but entire classes, thus giving a body of persons selected perfectly at random, and likely to yield average results.

It is possible, then, in spite of the various sources of error, to form a conclusion regarding the general frequency of the various phenomena under discussion, as well as in the different sexes and in those of different ages. In general terms, it may be said that the answers of the women show that they have less experience with the results of unconscious cerebration than men. Possibly this is explained by less observation of such matters in their case. Concerning the different ages, there is usually, where the element of the mind's development and increase in maturity does not enter, a decrease with increasing age in the frequency of conscious results of unconscious action, which is frequently interrupted between twenty-five and thirty years of age. This interruption occurs so often that it scarcely seems due to accident or error, and yet no adequate explanation for it can be offered here. It is possible that the decrease with increasing age is due to the decreasing plasticity of the brain molecules or brain cells under normal conditions. Aside from the special results concerning each question, it does not seem that the figures warrant any further or more definite conclusions.

EXPERIMENTAL PSYCHOLOGY AT WELLESLEY COLLEGE.

MARY WHITON CALKINS.

After the discussion of the relative merits of experimental as compared with merely introspective psychology, a practical question suggests itself concerning the introduction of experimental psychology into the regular college curriculum. This is a complicated problem of expediency, the question of the equipment of the laboratory, of the relative amount of laboratory work, of the proper direction of students' experiments. Such questions are especially prominent in cases in which psychology is a required subject, and in which our course is a general one and must be adapted to students without especial scientific training or without particular interest in experimental work. In such a course, it is sometimes urged, the introduction of experimental methods burdens the general student with details valuable only to the specialist, substitutes technical minutiæ for psychological principle and tends to confuse psychology with the other sciences.

This paper is an attempt to meet difficulties of this sort by the record of a year's experience with a general course in psychology, making extensive use of experimental methods. In the fall of 1892 a course in "Psychology, including Experimental Psychology," was offered at Wellesley College as one of the alternative senior requirements in psychology. The course was taken by fifty-four students, of whom all but one or two had had no previous training in the subject. All of these had taken a year's course, including laboratory work, in chemistry, and only three had failed to follow a similar course in physics. Most had no training in physiology, and many of them had a more or less pronounced distaste for laboratory work. The aim throughout was to supplement, and in no sense to supersede, introspection; to lead students to observe in detail and to verify facts of their ordinary experience; to familiarize them with the results of modern investigation and with the usual experimental methods, and to introduce them to the important works of psychological literature.

The first month was devoted to a study of cerebral physiology. Ladd's "Elements of Psychology" was used in this early part of the course as a text-book. The class work included recitations, informal lectures and some written work on the part of the students. One of these papers, for example, required an enumeration, accompanying a rough diagram, of the parts of the human brain, as developed from the dorsal and ventral sides respectively of the three "primary bulbs." The study of the brain by text-book, by plates, and especially by models, preceded the dissection by each student of a lamb's brain. The brains had been preserved according to Dr. James's directions.[1] (Wide-mouthed candy jars, fitted with rubbers to prevent evaporation, proved an inexpensive substitute for the regular Whitehall and Taitum jars.) The dissection was under

[1] Since published in the Briefer Course in Psychology, pp. 81 90.

the general direction of the instructor. The students were provided with simple directions and were required to identify the most important parts of the brain. The results of this work were very satisfactory. The students, even those who had dreaded the dissection, were practically unanimous in regard to its value, as clearing up the difficult points in cerebral anatomy. In the class room, during this week, in which the dissection was going on, the principal theories of cerebral localization were discussed.

The next six weeks were spent in experimental study of sensation. About seventy experiments were performed by the students on sensations of contact, of pressure, of temperature, of taste, of hearing and of sight. The experiments, almost without exception, were selected from those suggested by Dr. E. C. Sanford in his "Laboratory Course in Psychology,"[1] but re-arranged with reference to the plan of the lectures and of the class discussion. Papyrographed descriptions of the experiments were distributed to the students and commented on in class before the experiments were undertaken. The instructor kept daily laboratory hours in order to answer questions and to offer assistance. Each student was responsible for the record of her own experiments.

In class, reports were made on the results of experiments, and recitations were conducted on the physiology of the different senses. The bearing of the different experiments on the theory of perception was carefully discussed. Special effort was made to free the word "sensation" from the vague, dualistic meaning which it often carries with it; sensation was treated as essentially "the first thing in the way of consciousness." The three theories of perception, Associationist, Intellectualist and Physiological-psychological, were carefully studied, and in this connection parts of Dr. James's chapters on "The Mind-Stuff Theory," "Sensations" and "Perception" were assigned for reading. Of course, in so elementary a course no new experimental results were gained. All the more important experiments usually performed were repeated. The taste experiments were so unpopular that I should never repeat them in a general class of students who are not specializing in the subject. I should also omit most experiments involving exact measurement. For instance, I should do no more than familiarize the class with the use of the Galton bar and of the perimeter.

Some of the students were genuinely interested in the experiments, carried them further than required and made independent observations; a large number, on the other hand, performed them conscientiously, but without especial enthusiasm; some cordially detested them from beginning to end; but almost all recognized their value as a stimulus to observation and as a basis for psychological theory.

The following questions, asked at an informal, forty-five minute examination, suggest the character of the experimental work:—

I. Describe fully the following experiments. State the theories on which they bear and the conclusions which you draw from them:—

a. The "colored shadows" experiment.

b. Scheiner's experiment.

II. What are the dermal senses?

III. What is the (so-called) joint sense? Describe an experiment proving its existence.

In the study of association, the old distinction between association "by contiguity" and that "by similarity" was replaced by one between "desistent association," in which no part of the earlier

[1] American Journal of Psychology.

object of consciousness persists in consciousness, and "persistent association," in which all or part of it persists.[1] Dr. James's quantitative distinctions, corresponding with the terms "total," "partial" and "focalized," were also made. Students were referred to Hobbes, to Hartley, to Bain and to Dr. James, and were required to illustrate, by original examples or by quotation, the different sorts of association. This work proved very interesting and was valuable in co-ordinating psychological with literary study. The experimental work accompanying this study illustrated the value of association in shortening intellectual processes, and consisted simply in comparing the slower reading of one hundred unconnected monosyllables with the reading of one hundred connected words. Reading of passages of one hundred words in different languages was also carefully timed and compared.

A more extended experiment in association was later carried out. Each student wrote a list of thirty words, so associated that each suggested the next. The starting point was the word "book," suggested in writing, but not read until the time of the experiment. Each list was studied by its writer, who marked with a V the names of objects or events which were visualized; indicated with a C those connected with childhood life; classified the association, as desistent or as persistent (of quality or of object); and indicated, in each case, the so-called secondary law of the association (recency, frequency or vividness). Of course each list was written when the subject was alone and undisturbed.

The fifty papers thus prepared were carefully studied by one student as the foundation of her final essay. She compared with them similar ones obtained from two classes of school children of varying ages, and from a few people in middle life. Her results are interesting. One-half the objects or scenes (in the college lists, which were the only ones indicating this fact) were visualized. Only one-fifteenth related to childhood — suggesting that the predominance of Mr. Galton's childhood associations[2] is not universal. The lack of them may be due to the youth of the experimenters. A comparison of the lists shows that in the children's lists, recency is the most frequent explanation of suggestiveness, while in the college lists the vividness of the earlier object of consciousness is of greatest importance. The result tallies with the fact that "children's interests are more of yesterday and today."

The subject of attention was discussed on the basis of Dr. James's admirable chapter. The experimental work was in divided attention, the performance and accurate timing of two intellectual processes, first separately and then in combination.

A brief study of consciousness in its "identifying" and "discriminating" aspects was followed by a six-weeks' study of space-perception. Lectures were offered on the three chief theories, the Empiricist, the Nativist-Kantian and the Nativist-Sensational. The required reading included references to Berkeley, to Mill, to Spencer, to D. A. Spalding (MacMillan, February, 1873), to Preyer (Appendix C of Vol. II., The Mind of The Child), to James (parts of the Space-Chapter), to Kant (Æsthetic, "Metaphysical Deduction").

The experiments, of which there were more than thirty, illustrated the methods of gaining, or at least of developing, the space-consciousness. The theories of single vision were carefully studied and were illustrated by diagrams and by "Cyclopean eye" experi-

[1] Cf. an article in the Philosophical Review, July, 1892.
[2] Inquiries into Human Faculty, pp. 191-203.

ments. The study of the perception of depth included an adaptation from Hering's experiment, in which the subject, looking through a tube, finds that he can correctly distinguish, within very small distances, whether a shot is dropped before or behind a black string, stretched before a white background. The fact and the laws of convergence were studied with the aid of a Wheatstone stereoscope.

There followed a consideration of illusions of space; and of Unvisual space, including the experiments suggested by Dr. James on so-called tympanum spatial-sensations, and others, with a telegraph-snapper, on the location of sounds. The scope of the work is suggested by the following outline for a paper required at the close of the work, and written without consultation of books or of notes.

SPACE.—Review Subjects.

(NOTE: Support all statements by description of experiments bearing on the question.)

I. Theories of Single Vision.
  a. Identical Point Theory.
    1. Statement.
    2. Limitations.
  b. Physiological Association Theory.
    1. Statement.
    2. Relation to *a*.

II. The Third Dimension.
  a. 1. The Nativistic Position (in every form).
    2. " Empiristic " " " "
  b. How do we gain (or at least develop) the consciousness of the third dimension?

(NOTE: Include reference to the questions: Can we perceive depth without motion of the eyeballs? If so, what are the means of perceiving depth in the given case?)

III. The Value of the Empiristic Theory to the Nativist.

In the study of memory and of the imagination, the only experiments were a few on "The Mental Span." Students were referred to James, to Burnham, to Lewes, to Ruskin and to Everett. Paramnesia was of course discussed.

Abnormal psychology received, throughout the course, comparatively little attention, because it seemed so evident that a careful study of the facts of normal consciousness must precede any scholarly consideration of the abnormal; because, also, there seemed special need of combating the popular notion which apparently regards psychology as a synonym for hypnotism and telepathy. The abnormal was therefore treated throughout from the point of view of the ordinary consciousness and its phenomena were discussed as exaggerated manifestations of the phases of all consciousness. The subject was naturally introduced by a study of dreams; hypnotism was the only other topic considered.

The study of the emotions and of the will was accompanied by no experimental work. Chapters of Höffding, of James, of Mill, of Spencer and of Darwin formed the required reading; James's theory of the emotions was discussed; a classification of the feelings, adapted from Mercier[1], but rejecting his physiological principle of division, was the starting-point of a somewhat practical discussion.

The last week of the course was occupied with reaction-time experiments, which had been postponed to this time, only through necessary delay in procuring the apparatus; the work should properly have been scattered through the year. There was time for

[1]Mind, Vol. IX.

little more than an illustration of method and an approximate verification of the more important results in reactions to sound and in more complicated reactions, involving association, discrimination and choice. Averages of simple reaction-times, with and then without signal, showing a general increase in the time of the latter, were made by several students and included in essays on attention. Students were required to read Jastrow's "Time-Relations of Mental Phenomena."

The study of volition led to several days' discussion of the problems of determinism and indeterminism. This was undertaken with the express remark that the subject is metaphysical and not psychological. The favorable result of this study confirms my opinion of the value of an occasional consideration of so-called metaphysical problems in a general course of psychology, with students who are neither studying philosophy nor specializing in psychology. To repress the philosophical questions, suggested by a study of psychological phenomena, is difficult; to consider them is harmful, only if the distinction between metaphysics and psychology is not sharply made; to discuss them may be of great pedagogical value. One may agree with Dr. James that "metaphysics fragmentary, irresponsible, and half-awake, and unconscious that she is metaphysical, spoils two good things when she injects herself into a natural science," but one may still believe that a course which properly is called psychology, because of the immense preponderance of psychological discussion and investigation, may yet make an occasional metaphysical digression. Such a plan at least avoids the difficulty which may be raised in the cases in which the course is a required one in the philosophical department—the objection that psychology, as a mere science among sciences, does not merit this pre-eminence.

In place of a final examination, a psychological essay was required. The subjects assigned were very general and were intended as subjects for study rather than as definite essay-headings. The immediate topic of the paper was to be decided after the study and not before. Such subjects as "Association," "Attention," "Memory," "Imagination," "The Psychology of Language," "The Psychology of Childhood," "The Psychology of Blindness," "Aphasia," "Animal Psychology," were chosen in this way. Writers on the first two subjects worked up the statistics, to which allusion has already been made. The specialization of topics, within the broad field indicated, showed itself in various ways. One student studied the prevalence among imaginative people of a "continued story," never written, seldom mentioned, but very significant, especially in the emotional life. Of fifty-four students who were questioned, nineteen have had such a story, whose characters grow, but retain a constant identity; whose situations change, but only in accordance with the plot, usually a simple one, of the story. With all the story began in childhood; with eight it still continues, but in all cases the interests of adult life have somewhat overshadowed it. A comparison was made with the experiences of thirty-six young men, students in Iowa College, with the result that only three testify to the possession, at any time, of a continued story.

The study of the psychology of blindness was accompanied by visits to the Perkins Institute. A student who writes on "The Imagination of the Blind," bases her conclusions on a personal study of twenty-five blind children. She questioned the children, consulted with their teachers, and read their compositions. Those who write on the psychology of childhood have made personal observations on babies and little children. One writes on "The

Senses of the Child," the other on "The Memory and Imagination of Chilldren." Both papers contain fresh material.

No topics in abnormal psychology were originally included, but several students especially interested in the subject write on "Hypnotism," "Dreams," "Illusions." Most of these kept written records of their dreams, during two months. Some interesting observations were made. One dreamer was able to reproduce, in a series of drawings, certain figures of a dream; several well-marked instances of reasoning are noted; one writer makes the discovery that "my dreams are forgotten immediately upon waking, but that the instant I touch my head to the pillow the following night, the dreams of the preceding night come back with great clearness. In keeping a record, I was often obliged to wait until the following night to record something I had dreamed early in the morning." The analogy with the hypnotic memory is very interesting.[1]

In connection with the work of the course, a collection of statistics about colored hearing and number-forms was undertaken. Five hundred and forty-three persons, of whom five hundred and twenty-six were members of Wellesley College, were questioned. The general results are the following: Ninety-eight are affected in one or both of these ways; thirty-two have colored hearing, and seventy-eight have forms for numbers, for months, for days (or for all); fourteen have both colored hearing and some "form." These facts were gained by students; aided by a simple set of questions; in the cases of colored hearing, results were verified by questioning the subjects a second time after the lapse of two months; the number-form was in each case drawn by the subject; in all cases, records have been kept in uniform shape. Five essays were written on the basis of these statistics, of which each makes a special study of one or two cases of particular interest; from these essays I make occasional extracts in my brief report of our results.

Among the thirty-three cases of colored hearing are nineteen in which proper names of people suggest a color; nine instances of musical association; twelve cases in which names of days or of months are associated with color, and four of colored number-association, of which one is a colored number form. In seventeen cases letters suggest colors, usually by their sound, but sometimes by their appearance, and in three of these lists, the whole alphabet is included.

Galton's assertion that vowels more commonly than consonants suggest colors, is not confirmed by our results; among seventeen letter-associations, there are ten in which both consonants and vowels are suggestive; four in which consonants only are associated with color; and but three in which only vowel-associations occur.

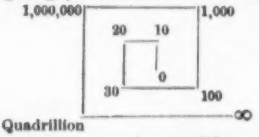
The manner of the word-coloring cannot be reduced to any general formulæ. It follows sometimes the color of the initial letter, sometimes that of a repeated letter. Sometimes, again, its color is that of the mixture of the colors of the different letters. In one case, "0 by itself is gray; when occurring with other numbers it takes the color of the accompanying number. Thus 80 is blue, the color of 8."

Like most observers, we have found that o and i correspond respectively with white and black — and this is almost certainly

[1] Cf. Ribot, Diseases of Personality, p. 118 (I know no other reference to this phenomenon).

through association with the appearance of the letter. The only positive exception which we find[1] in which *i* is red, falls in with the more indefinite generalization of MM. Beaumis and Binet[2], that either *i* or *a* is black, white or red. We find also the following instances of apparent uniformity: In six cases out of twelve, *a* is blue; in five out of twelve, *e* is yellow, and in six out of thirteen, *s* is yellow. This last is the only instance of uniformity among consonant-associations.

Our recorded number-forms, of which there are forty-eight, are of most varied sorts, including single and parallel lines; lines horizontal and vertical; zig-zags, curves and one Greek border,



One subject has "two forms, one for positive numbers to infinity, and another (extending in both directions from 0) including negatives, infinitesimals and fractions."

Not all changes of direction are at 12 or at 10. "Out of twenty-four, in which the lines are continuous,

Seven	turn	at	10
Six	"	"	12
Five	"	"	20
Five	"	"	6"

About two-thirds turn toward the right. Several forms are in tri-dimensional space, and we have one elaborate description of a form stretching away from the subject, in which the more prominent numbers stand out like mountain-peaks and hide the intervening ones.

Our records include several cases of pronounced emotional association combined with a sort of dramatization of numbers or of colors. Thus, one subject writes, "1, 2, 4, 7 and 8 are reliable, quiet, well-disposed, but not brilliant numbers; 3 is a sharp, shrewd, noisy and disagreeable number, always making as much trouble as possible. For 13 I always had a great antipathy. It had all the disagreeable qualities of 3 added to a pertness and aggressiveness which made it repugnant to all the other numbers, with which it seemed never to associate. I never wanted to be thirteen years old."

The circle is the most constant of the month-forms (occurring in eighteen out of fifty cases). Its plane is in most cases parallel, in one perpendicular, to the plane of the earth. In the forms for the days of the week, Sunday almost always occupies a conspicuous position. "When the plane changes, this day is higher than the rest, so that in passing from Saturday to Sunday, a step up is made." To one person "the first three days of the week seem much greater than the last three. In vacation, I always plan to make visits, receive company on the first three days of the week; the other three seem crowded together and insignificant."

No new explanations of the phenomena were offered or discussed. Only one affirmative answer, among twenty in the negative, was received to the question: "Can you compare your form with any

[1] There is, besides, one case in which *i* is grey.
[2] Revue Philosophique, April, 1892.

pattern of wall-paper or carpet, with any crack in the plastering or with any other line which you might have seen when learning the numbers in childhood?"

So far, then, as our results support any theory, they tend toward one that is physiological; at least, they oppose, especially with reference to colored hearing, the explanations through any other than forgotten and fundamental childhood associations.

PSYCHOLOGICAL LITERATURE.

I.—ACTION AND VOLITION.

By Professor J. M. Baldwin.

CHAUVEAU: *On the Sensori-Motor Nerve-Circuit of Muscles; Brain.* LIV. and LV., 1891, p. 145 ff.

This paper is an experimental attempt to establish a so-called Sensori-Motor Nerve-Circuit for the Muscles, i. e., a direct inmovation of muscle due to a current passing up from the muscle by the sensory branch and down by the motor branch of its nerve. For this inquiry Chauveau uses two muscles of which the sensory nerve courses are isolated from the motor, the sternomastoid (voluntary) and the muscles of œsophagus involved in deglutition (involuntary). In the case of sternomastoid, Chauveau finds, operating on the horse, (1) excitation of the motor branch gives contractions with no sign of sensibility; (2) excitation of the sensory branch gives contraction, also slightly delayed. Conclusion: A connection between the central ends of the sensory and motor courses, i. e., a reflex "circuit." The same result appears when the branches are respectively cut. Excitation of the end of the motor branch which adheres to the muscle gives contraction; excitation of the corresponding sensory branch end gives no result, and excitation of the end of the fixed motor branch gives no result, of that of the fixed sensory end gives contraction; so that the commotion always travels one way in the circuit, up the sensory and down the motor branch. The only differences in the stimulation of the muscle from the centripetal as against the centrifugal course are that the former is delayed in time and requires a stronger current. Testing the same muscle by simple section, i. e., by the withdrawal of physiological support rather than by positive electrical excitation, the results are the same, except in one particular. As would be expected, cutting the motor nerve gave paralysis of the sternomastoid, but cutting of the sensory branch had no effect upon the muscle. "Section of the sensory branch does not abolish movement," and "I failed to obtain evidence of any alteration of function." The same experiments on involuntary muscles—those of œsophageal deglutition—gave the same results, except that last named. Here section of the sensory branch produced "sometimes transitory and irregular paralysis, sometimes a peristaltic incoordination." Chauveau notes that the difference between voluntary and involuntary muscles in respect to the effect of the absence of sensory nerve support contradicts the results of Claude Bernard, who by cutting a series of lumbar sensory roots in the dog obtained motor disturbances in the corresponding hind limb. He explains the contradiction on the ground of differences in the relative complication of the muscle in question with others in performing coordinate functions. The motor branch in one case may be excit-

able through its connection with the sensory branch at the same time that it is not dependent upon the sensory reports of the latter for its inmovation: its connections with other sensory functions may still be sufficient. But in another case (Bernard's) it may be both excitable through and dependent upon the sensory reports.

It is evident that there is another possible explanation, i. e., that while sensory reports are sufficient for the regulation of involuntary movements, they are not sufficient for voluntary movement; these latter are regulated from a higher co-ordinating centre. This alternative Chauveau suggests and rejects, although he admits the physiological difference it requires, i. e., "one (muscle) is automatic, not influenced by the psycho-physiological centres, the other is a part of the voluntary locomotor system, influenced by these centres." The difficulty with his explanation is three-fold: (a) If the dependence of the motor function in question upon the support of the sensory branch be inversely as the degree of implication of other sensory connections, then the most complex and co-ordinated function, i. e., walking, would be most free from impairment when certain of these sensory connections are cut. Such relatively simple function as moving the head up and down upon the neck—due to the contraction of the sternomastoid—we would expect to be most impaired by cutting the sensory branch. (b) We may ask why such sensory support from other connections does not also avail to prevent impaired function in the case of the involuntary muscles? (c) Even though the kinæsthetic regulation by groups of sensory nerves be allowed, we would expect some impairment of function when the entire sensory contribution from the muscle in question is cut off; the amount of laming would be a matter of degree. But this is contradicted both by the result on the sternomastoid, and by Chauveau's experiments in cutting the four nerves supplying the toes of pigeons. He found that "complete sensory enervation of an extremity does not appreciably disturb these (the locomotor) functions," and "they roost indifferently upon the normal and upon the enervated foot." I may add, also, that these experiments upon the sternomastoid are valuable for the discussion of the regulation of voluntary movements, since all kinæsthetic support from "remote" sources (the eye, ear, etc.) are here ruled out.

Other interesting results are: Contractions of facial and lingual muscles by stimulating the nuclei, in the fourth ventrical, of the seventh and twelfth pairs; contraction of abdominal and spinal muscles by stimulating sensory cells of cord between the last dorsal and first lumbar root, after section of the cord from the brain (artificial respiration being carried on); co-ordinate respiratory movements in horses, after separation of the medulla from the encephalon, by stimulating the intercostal nerves. As to the mechanism of the sensori-motor circuit thus demonstrated, Chauveau criticises the ordinary kinæsthetic theory as being too complex, and adopts the view that there is a repropulsive wave which runs toward the muscle along the sensory course, reaching the muscle simultaneously with the motor impulse, and regulating the muscular contraction.

WALLER, *The Sense of Effort; An Objective Study; Brain.* LIV. and LV., 1891, p. 179 ff.

Dr. Waller attempts to ascertain by experiment the locus of the sensation of fatigue after muscular work and thereby also the locus of the sensation of muscular effort; holding that the former is related to the latter as an after-effect to a first-effect, analogous to

the relation between retinal images and after-images in vision. The main results on fatigue are as follows (he gives details of experimentation, apparatus, graphic records, etc., for which the article must be consulted): (1) The nerve courses do not contribute to fatigue. (2) There is no diminution in the lateral enlargement of a muscle from direct (artificial) stimulation after a series of maximum voluntary contractions, i. e., voluntary fatigue has not exhausted the muscle. (3) If a maximum series of voluntary effects be interpolated between two maximum series of direct effects, the second series of direct effects suffers no diminution, i. e., there has been recovery in the muscle for direct work during the series of voluntary effects. (4) The same recovery of the muscle for voluntary work during direct stimulation is seen by interpolating a series of direct effects between two series of voluntary effects. Waller agrees with Mosso, who obtained similar results, in holding that this recovery can only be in the peripheral organ in the former case and in the central organ in the latter case. (5) There is no diminution in voluntary effects after a maximum series of direct effects. (6) In the longitudinal effect there is a diminution both in direct maxima during voluntary work, and (less) in voluntary maxima during direct stimulation: a result which again agrees with that of Mosso. The general conclusion is that "voluntary fatigue depends more upon central than upon peripheral change." Further, the lateral (rigor) effect, in voluntary contraction of fatigued muscles slightly precedes and outlasts the longitudinal effect; this delay in the subsidence of the lateral effect is mainly due to a continued action of the centre upon the fatigued muscle, "a contraction reminder," "a residual discharge from the fatigued centre." Further, the cessation of voluntary longitudinal effect is quicker in fresh than in fatigued muscle. These results follow the use of a spring dynamometer rather than the weight-lifting method of Mosso. Waller criticises the latter on the ground that a weight is often lifted by more than the minimal effort really necessary to lift it. This criticism is proved by the fact that the weight method applied to the last mentioned experiment gave a longer persistence of the longitudinal than of the lateral effect, showing that some strain was relieved before the longitudinal effect actually produced by the weight was touched. The same criticism is proved to hold also *mutatis verbis* for the beginning of a weight-experiment, i. e., some force is spent before the weight begins to rise.

Waller also experiments on the flying-up of the arm when suddenly released from a voluntarily-lifted weight. He finds (using Douder's apparatus) that the upward spring of a fatigued is greater than that of a fresh arm. This agrees with Mosso's result that in a succession of such "releases" the amount of spring progressively increases. Waller explains the fact by the result given above that there is a "contraction reminder," which gives delay in the cessation of the nervous discharge in consequence of central fatigue. (This accounts both for the fact of such an upward spring and for the fact that it increases with fatigue; while Mosso's explanation, i. e., that greater stimulus is sent to the muscle in consequence of its fatigue, only accounts for the second fact.) A series of experiments is reported, further, on the estimation of weights when the muscles of the hand are "directly" stimulated. He found, in opposition to Ferrier, Goldscheider and Bernhardt (who, however, Waller claims has been misquoted in this connection), that under cathartic excitation of the digital flexors by the median nerve, 1000 gr. was barely distinguished from 3000 gr., or 500 gr. from 1500

gr., while, when voluntarily lifted, 1000 gr. and 1500 gr. were distinguished. In the case of faradic stimulation, practically no discrimination of weights was possible. This removes one of the strongest experimental arguments for the exclusively peripheral estimation of weights.

Ingenious as Waller's main argument is, it leaves two loop-holes of escape from his conclusion: (1) Granted that in fatigue the centre is mainly exhausted, it does not follow of necessity that this centre is the point of "incidence" of the feeling of fatigue. Mosso shows that central exhaustion has a chemical peripheral effect; this may occasion the feeling of fatigue. Or central expenditure may draw a kinæsthetic centre apart from itself, this latter contributing the feeling of fatigue. Waller's inference is a highly probable one, but not the only possible one. Further granted for the moment that the inference is correct, it only puts the sense of fatigue somewhere in the centre, not necessarily in the motor seat, unless we agree with Waller in denying the distinction between sensor and motor rests. (2) Granting the full value of the experiments, they again go no farther than to render probable the point of "incidence" of the sense of effort. Such a feeling may still arise in a kinæsthetic centre in dynamic connection with the working motor centre, or from "remote" peripheral courses. Nevertheless, logical alternatives aside, Waller's conclusion is undoubtedly the best interpretation of Waller's premises.

DELABARRE, *The Influence of Muscular States on Consciousness; Mind.* N. S. 3, July, '92.

This paper is largely a summary of the author's *Ueber Bewegungsempfindungen*, which will be noticed later. In this connection, only Delabarre's criticism of Waller may be referred to. Delabarre claims that Waller's argument contains three assumptions: (1) "That the objective signs of exhaustion are always indicative of a previous expenditure of energy in the same parts." [Not so. Waller uses the same muscle for voluntary and direct excitation. His assumption is: Granted both voluntary and direct work from the same muscle and objective exhaustion only for voluntary work, then the objective exhaustion must be in the nervous centre for this muscle. This is valid, and Delabarre's statement is a fallacy of conversion.] (2) "That a subjective sense of fatigue is indicative of a corresponding previous effort, and sense of effort, in the same parts." [Not so. Another involved conversion. Waller's position is: Granted a maximum voluntary effort and sense of effort directed to a part, and a sense of fatigue following the exercise of the same part, then the seat of this sense of exhaustion is the same as the seat of the sense of effort. This does not necessarily follow, as I have said above, but is physiologically extremely probable.] (3) "That objective signs of exhaustion are indicative of a subjective sense of fatigue, and objective signs of effort of a subjective sense of effort, localized in the same parts." [I also find this assumption, and have above expressed an opinion of the extent to which it invalidates Waller's conclusion.]

MÜNSTERBERG, *Mitbewegungen, in Beiträge zur experimentellen Psychologie*, Heft IV., 192.

Professor Münsterberg gives a résumé of the discussion on the question of the concurrent innovation of symmetrical movements on the two sides of the body, against the old view that there was a direct tendency to such symmetrical movements when either side

is stimulated; he opposes both theoretical and experimental considerations. His experiments, executed in co-operation with several of his students, consisted in the voluntary performance of preconceived drawing and tracing movements (circles, triangles, squares, etc.), with one or both hands; then in occupying the attention with one hand only and observing the behavior of the other hand; or in withdrawing the attention altogether and observing the behavior of both hands, etc., through the various combinations available. His results lead him to the conclusion that in movements up and down and away from the body, such symmetrical movements do not occur; but that in movements right and left from the body such movements do occur. These latter cases he explains as due to the maintaining of the equilibrium of the body. His conclusion is that "inborn symmetrical co-ordinations of the muscles of the extremities do not exist." [This conclusion is by no means proved by M.'s interesting experiments. In the first place, the present writer finds it almost impossible to keep the attention so constant as not to interfere with the so-called "Mitbewegungen." Further, if Waller's result be true, there would be a certain residual discharge in the muscle after voluntary attention is withdrawn from it and this ought to give a certain amount of movement, symmetrical or asymmetrical, according to the voluntary movement. It is quite possible that there is such an element, but that it is drowned in the grosser tensions due to equilibrium, maintenance of balance, required habit, etc. Such a tendency could only be measured by a graphic record under conditions which ruled out the grosser sources of error; not by the rough explanation of a group of people standing or seated around a table. But, more than this, is experiment on adults likely to throw any light on this question at all? Everybody admits that our adult movements are massed in asymmetrical co-ordinations, which represent the strongest dynamic tendencies. There are, also, facts on the affirmative side of the question, such as "crossed reflexes" (see this Journal V. '92, p. 84). In the case of infants we have "crossed" responses in sleep (see my observations in Science XIX., 1892, p. 16, and Preyer's Mind of the Child, I., p. 207 ff). M.'s citation (p. 195) of my observations in connection with the development of right-handedness (resumed above) does not take account of the fact that in reporting these observations I added, a little later on, "In many cases the left hand followed slowly upon the lead of the right;" this was also true in the cases in which the left hand led—the right followed after in the same direction. A crucial test of the general question might be reached by Gotch and Horsley's new electrical method; the galvanometer showing to what extent, if at all, the cortical stimulation of a muscular group affects the motor nerves of the opposite side. As far as Gotch and Horsley's results on cats and monkeys bear on the question, they indicate that the tendency to bilateral performances is relative to the intensity of the stimulation —what we would expect from the general principle of diffusion. (Gotch and Horsley, "On the Mammalion Nervous System, etc.," Croonian Lecture, Phil. Trans., 1891.)]

SCHENCK, *Uber den Erschlaffungs Process des Muskels Pflüger's Archiv*, LII., 1892, p. 117.

S. asks the question: Why is it that the down-slope of the curve of contraction of a muscle exhausted by cooling is as steep as its up-slope, while the down-slope of that of a voluntarily exhausted muscle is not as steep as its up-slopes? He surmises that the less

abrupt down-slope of the voluntarily fatigued muscle is due to the greater consumption of its "reserve elements," apart from the production of lactic acid (I. Rauke). He asks the question, accordingly, whether the relaxation of a muscle in general takes place more slowly when its reserve elements are fewer. This he investigates by an experiment, the conditions of which are to compare the curve of a voluntarily fatigued muscle with that of a muscle whose excitability is reduced by lactic-acid artificially, i. e., in such a way as not to reduce the "reserve-elements" of the preparation. He injected the gastrocnemius of a frog with a .125% lactic acid solution. Its down-slope, like that of the cooled muscle, was less steep than that of the normally fatigued muscle. The same resulted with muscles injected with .1 to .2% soda. The rest of the paper is devoted to a discussion of theories of the molecular processes of muscular contraction and relaxation.

SCHENCK, *Ein apparat zur Verzeichnung von Länge und Spannung des Muskels*, Ibid, p. 108.

The title of this article indicates its contents.

WALLER, *On the Inhibition of Voluntary and Electrically Excited Muscular Contraction by Peripheral Excitation, Brain*, LVII., 1892, 35.

Waller asks whether the diminution in the force of a voluntary muscular contraction brought about by the superposition of direct electrical excitation is due to central inhibition (Fick), or to peripheral inhibition in the body of the muscle (Mosso), or to the excitation of the antagonists. Experimenting on the flexor muscles of the forearm and taking records, both by the dynamograph and "bag recorder" (see description in Brain, 1891, 206), he finds that there is a diminution in the maximum voluntary longitudinal effect, due to the electrical stimulation, but an increase in the maximum voluntary lateral effect. Further, that voluntary contraction superposed upon maximum direct foridization increases both the longitudinal and lateral effects, but that while the sense of maximum direct plus voluntary longitudinal effects is less than the maximum voluntary alone, the sense of the maximum direct plus voluntary lateral effects is more than the maximum of either taken alone. He explains these results largely by the stimulation of the antagonists (extensors); supporting this view by researches on the elevator muscles of the lower jaw, which have no antagonists (in which case the phenomenon in question does not appear), and on the flexors and extensors of the arm (which when both directly stimulated reproduce the phenomenon). A farther question is: Does the cessation of voluntary contraction involve simply a cessation of central voluntary emission, or a stimulation of the antagonists? Waller holds that the former is the main effect. He finds that in cases where the antagonist (extensor) is contracted, there is, on the release of the muscle (flexor), a prolongation of the lateral effect as compared with the longitudinal effect; but in voluntary release of the flexors, there is no such prolongation of the lateral effect. He also finds that in simultaneously grasping with one hand and letting go with the other, the two effects (curves) begin to appear simultaneously; but if the agent in each process be foridization, the muscle stimulated begins to contract before the released muscle begins to cease contracting. Turning from the effects of induced to those of galvanic currents in connection with voluntary contraction, he reaches conclusions which confirm Pflüger's and Waller and De Walleville's earlier results (Phil. Trans., Royal Soc., 1882). The general result is that "active arrest of action, i. e., true physiological inhibition of voluntary muscle, has not yet been demonstrated."

GENERAL MISCELLANEOUS.

EUGENIO TANZI. *Cenni ed esperimenti sulla psicologia dell 'udito. Rivista di Filosofia Scientifica.* December, 1891.

One-half of this article is a review of theories, physical, physiological, and psychological, on sensations of sound. The second half is an account of the author's experiments on the reaction-time for major and minor chords. The keys of a pianoforte, the reagent's key and the chronoscope were joined in one circuit. The results are shown in two tables, the one for simple reaction-time, the other for choice between reaction and no-reaction. Conclusion: The minor third has a "character of greater evidence" than the major, but is not always more quickly perceived. E. PACE.

SIEBECK, *Beiträge zur Entstehungs-Geschichte der neueren Psychologie.* Giessen, 1891. S. 34.

This includes three studies on the growth of psychology during the perion between Aquinas and the Renaissance. The first is devoted to Eckhart, the other two to Buridan. As an advance upon his scholastic predecessor, E.'s chief importance lies in his treatment of the feeling side of mind. His recognition of an inmost soul-phase which blends subject and object, foreset and will in a higher unity is at once a departure from the old classification and a step towards the new. His mysticism asserts itself in the contest which he assigns to this unity, or "Funklein," viz., the knowledge of God and of His indwelling in man. Still, his conception of the "Gemüthsleben" is an unmistakable prelude of modern teaching, and his insertion of the "Minne" between thought and will suggests quite plainly the tripartite division of mental phenomena.

Buridan's labors are marked by independence of thought and a preference for empirical methods. These traits are shown especially in his psychology, which is generally under-rated or minimized to the fable of the ass—whereof there is not a word in his writings. His doctrine of free-will is, in its earlier stage, an echo of the traditional "libertas oppositionis," the power to choose between opposites for which the motives are equally strong. But this appears later on simply as a means of attaining ethical freedom—the "libertas finalis ordinationis." To effect the transition, Buridan narrows freedom to a power of suspending judgment of the intellect upon its presentations, until, by further investigation, that real good be discovered, which claims the assent of the will.

More interesting for modern psychology are the "beginnings of a psychical mechanics" discernible in B. His attempt at a simpler classification of sense perceptions, based on genetic principles, breaks ground for the Associationists. In his answer to the question whether out of several simultaneous presentations each is as clearly perceived as though it appeared singly in consciousness, he takes into account the intensity and contrast of such presentations and the direction of the attention.

The "Relativity of Sensations" is one of his principles. Weber would have welcomed B.'s observation that not every slight increment of stimulus produces a corresponding increase of sensation; and Goethe might have found the germ of his color-theory in Buridan's treatment of light and shade. Add to this his careful analysis of feelings pleasurable and unpleasurable, and of their interaction under varying intensity, and it will be clear that B., whatever effect he may have had on his contemporaries, was not far from the line of thought which psychology follows to-day.

Ueber den Hautsinn. De phil. et med. Max Dessoir. Du Bois-Reymond's Archiv f. Phys. 1892. Heft III. and IV. pp. 177-339.

In a theoretical introduction of seventy pages, the author discusses the concepts of sensation and perception, the qualities of sensation, associated and after-sensations, the law of specific energy, the objectification of perceptions, and the classification of perceptions. The theory of specific energy in the sense "dass ein und derselbe Reiz diese verschiedenen Wahrnehmungen hervorbringen könne und dass eine Mehrheit von Reizclassen * * * ein und dieselbe Wahrnehmungsart erzenge" is held to be false. (P. 245.) In the restricted sense that: "Eo kommt einem jeden Sinnesapparate eine specifische Erregung, jedem grosshiruniede bezirke eine specifische Function zu" (p. 281), the principle is accepted. As regards the "objectification" of sensations, the author joins the group which holds that externalization is finitive, and the reference to an ego derived. (P. 224.)

The results of the long and often heroic experimental study of the temperature sense (p. 246-339) which must, in the outset, be recognized as very unequal in scientific worth, are categorically as follows:

The temperature sense is one as regards modality, heat and cold being the two qualities of one and the same sense. There are no hot and cold spots. There is but one kind of end apparatus, and this can be acted upon by only one kind of stimulus. Weak to moderate temperatures cause pleasurable feeling; extreme or intermittent ones, harmful feeling; continuous stimuli give sensation of fluctuating intensity. Of several successive stimuli, the first are felt moderately in comparison with following ones; but still later stimuli are felt less strongly again. D. assumes that the time which elapses between the feeling of heat or cold and the feeling of pain, when extreme temperatures are applied, is a measure of the intensity of the temperature sensation, and repeatedly makes use of this assumption in his proofs. Stimulation of a sensory nerve by heat, cold (263), pressure, or electricity (266) causes no sensations of heat or cold; or if the last-named causes such sensations, this is to be explained by vaso-motor modifications and not by the customary theory of specific energy. (P. 269.)

Probably the "free endings" in the lower layers of the epidermis are the end organs of the temperature sense, though this is not proved. (P. 280.) The lower part of the œsophagus, the stomach, the mucous membrane of the regio respiratoria, etc., are insensible to temperature stimuli, and afford means of determining the temperature end organs by comparative histological examination. (Pp. 275-280.) The Gyr. sigm. is the cerebral centre for temperature sensations. (P. 283.) In six pathological cases, the quality and degree of temperature sensations were recognized, but not their location (p. 284). There is no proof of distinct hot and cold nerves, and the "beautiful hypotheses" of different resistances in the different fibres are "built upon the sand." (P. 285.) Burn wounds are sensitive to temperature or not according as lower layers are present or not. (P. 287.) Long continued heat or cold reduces sensibility. Galvanic electricity does not affect it. Kal. brom. and chloral hydrate taken internally increase—cognac and caffeine first increase, then decrease temperature sensibility. External application of five per cent. solution of cocaini mur. to mucous membrane made the part anæsthetic for pressure and pain without affecting temperature sensibility; while a twenty per cent. solution applied to the skin produced no effect; .005 grm. of a ten per cent.

solution of the same, injected subcutaneously, produced anæsthesia for temperature, later for contact, touch, electricity, pain and rendered "a small elliptical outer zone" hypersensitive for all these stimuli. Similar results were obtained with .015 grm. morphini hydrochlor. (P. 288.)

Mustard plaster produces hyperalgesic and reduces temperature sensibility. The result of Adamkiewicz, that sensibility for touch and pain is increased at the place of stimulations by mustard plaster and decreased on the corresponding part of the other side of the body, and that no such bilateral transfer of effects occurs in the case of temperature sensibility, is not tested, but is rejected by the author.

The median line of the body is less sensitive than either side, the right side than the left. The dorsal has a lower threshold, but a less fine discrimination than the volar. Discriminative sensibility is greatest on the back of the upper arm, least in the middle of the back; is not much affected by the normal temperature of the skin; stands in no clear relation to the thickness of the skin; varies within smaller limits than the sensibility for pressure and distance in different parts of the body and does not have its maxima and minima at the same places as the pressure and space senses (p. 293). The points at which temperature sensations pass into pain are from +48.7° C. to +58.6° C. for heat and from +2.5° C. to +4.1° C. for cold, according to the duration of stimulus and the part of the body affected. (P. 294.) Temperature stimuli are recognized as distinct at from 1.2 mm. to 6 mm., according to part of body. Increase of the surface stimulated increases the intensity of the sensation. Discrimination is best between 27° and 32° C. After-images are removed by the opposite stimulus and reinforced by repetition of the same stimulus. Weak stimuli give intermittent after-images. The continuous after-image of a strong stimulus lasts from 316 to 715 σ; for high degrees of temperature, producing pain, 1513 to 1,889 σ. Long continued stimulation wearies; brief stimulation refines sensibility for the opposite stimulus. Successive contrast is stronger than simultaneous. Successive and simultaneous contrasts supplement each other. A more intense stimulus may be replaced by a less intense stimulus applied to a greater surface.

As between minimal temperature (p. 303) stimuli and minimal contact stimuli, one may be in doubt which has been applied, but this does not prove that touch and temperature sensations are homogeneous. Cold objects feel heavier than warm, one to two and five-tenths grams having been added to the warmer of two thalers to make their weights seem equal. Cooling a part of the skin increases its sensibility for pressure stimuli. (P. 306.) For the determination of reaction time, the author devised a modified form of the Siemens-Pflüyer Fall Hammer, a "Finger-Contact" and a "Reizapparat" permitting variation of the intensity of stimulus and surface touched. (Pp. 306-311.) The author cannot report whether he and his subjects used the sensory or muscular reaction, because none of the gentlemen made the distinction. "The true reaction stands in the middle of these artificially developed extremes. (P. 312.) Reaction time for touch decreases with the increase of the surface touched and varies for different parts of the body; the point of the index finger having the longest; the neck and left cheek, the shortest reaction time, of several places examined. (P. 316.) Reaction time for temperature varies with the part of the body and the extent of surface touched; 215 to 951 σ time elapses between the sensation of contact and the sensation of cold when a drop of water is permitted to fall on the point of pulsation of the left radial

artery, the time increasing with the rise of temperature. (—10° to +20° C.) (P. 318.) A "preparation time" of two seconds is most favorable. (P. 322.) The topography of the temperature sense as regards reaction time, as given by Goldscheider (p. 317), is correct. The temperature reaction times as given by Goldscheider, Tanzi and Herzen (350 σ—750 σ for heat, 250 σ—600 σ for cold), are agreed to, while the much shorter times found by Vintsbgan are discredited.

As to the processes in the nervous system accompanying temperature sensations, the author rejects Lotze's "Oscillations," the Fick-Wunderli hypothesis that temperature and other skin sensations arise from different combinations of the same elements, the Blix-Donaldson-Goldscheider theory of hot and cold spots, and the Hering theory of assimilation and dissimilation. Instead of these, he awaits the "patient labor of long years."

In attacking more than a dozen theories in the field of the temperature sense, many of which have strong support, Dr. Dessoir shows very praiseworthy courage. Not so praiseworthy is the slight consideration or denial of facts alleged by men whose work is considered of the first order, when those facts stand in the way of the author's theses. For example, to facts alleged by Herzen (p. 281), and again to facts alleged by Adamkiewicz (p. 289), the author makes little other reply than that he does not believe them; and his explanations of facts, fundamentally controverting his own positions, reported by Blix, Donaldson, Goldscheider, Du Bois-Reymond, Rittes, Vintochgan and others, seem to an unprejudiced critic sometimes trivial, some times mutually contradictory, certainly inadequate.

All this is very sure to bring back upon the author a polemic as severe as his own. He will be the less able to meet counter attacks because of the serious inequality of his own work. For example, upon the critical question whether electrical stimulation of the sensory nerve causes peripheral temperature sensations, the author throws out 1017 of 1200 experiments, without giving any reason except that only the 183 retained were free from error (p. 268).

In page 300, experiments are reported in which the observer's auditory reaction is eliminated from the gross times obtained. This the author does by subtracting from the gross times the average reaction time given in Jastrow's Time Relations. This is so inexcusably bad that one wonders that the author thought it worth while to eliminate the observer's reaction time at all.

Of less importance are the objections to the author's special reaction time studies. His claim that the error in his sensibolometer is constant, when no guarantee is given as to the force and rate of its application, must be doubted. For the same reason doubt is cast upon the tests made with that instrument to determine the influence upon reaction time of the amount of surface stimulated (p. 313) and of the part of the body stimulated (p. 314).

On the other hand, the undoubted merits of this long and patient research will be recognized by every competent reader. The facts reported, as also the polemic which the author has mixed up with the report of facts instead of separating therefrom as one could have wished, will surely lead to a more general and thorough study of the questions involved.

W. L. BRYAN.

DERCUM, F. X., M. D. Note on a Chinese Brain. Journ. of Nervous and Mental Disease (New York), New Series, Vol. XVII. (Sept., 1892), 691-695.

Professor Dercum here presents his notes of an examination of "the brain of an adult Chinaman of the coolie class," comparing it with the only other Chinese brains treated scientifically, viz.: one by Mills and Parker in 1886, three by Moriz Benedikt in 1887, and two by Professor Dercum himself in 1889. He says in conclusion, "These brains, owing probably to the unusual sinuosity of some of the fissures, together with the excessive transverse fissuration, have a physiognomy, as it were, of their own. They certainly, in general appearance, look different from the average white brain that we handle, and very different from the brain of the negro."

A. F. CHAMBERLAIN.

THE LABORATORY OF THE PSYCHOLOGICAL INSTITUTE AT THE UNIVERSITY OF GÖTTINGEN.

By William O. Krohn, Ph. D.

This laboratory is in many respects the best for research work in all Germany. It is peculiar in that it owes its excellent equipment to a liberal gift from a private individual, the state giving but a mere pittance to its support. To the generosity of a former student and friend is Professor Müller indebted for the laboratory of which any university in any land might be justly proud. Not only is the apparatus entirely new, but it is exceedingly well constructed. The rooms so recently set aside by the curator of the University for this laboratory are so well adapted to the purpose of research and of such generous size that the old time objection of "limited space" can no longer be urged against the Psychological Laboratory at Göttingen. Besides a very large auditorium, they have three other large rooms, well fitted for different lines of research work, and a well aranged dark room—indeed this dark room is an ideal one. With the new commodious quarters and their carefully selected equipment, Professor Müller and Dr. Schumann are well equipped for guiding a large number of students in experimental work. Professor Müller's investigations are well known and Dr. Schumann has recently distinguished himself by some important pieces of work. He is also a skillful mechanical contriver and every one of the old standard pieces of apparatus in this laboratory (e. g., the control hammer) has undergone some improvement. He is a very ambitious man, and most worthy of the best success. He certainly has a remarkable future. Like Müller, he aims at accuracy and thoroughness rather than the accomplishing of a large amount of poorly done work.

There is also an interesting historical point connected with the Göttingen Laboratory. It is this: Professor Müller is the successor of the renowned Lotze, and it was Lotze who, in his lectures and published works, gave such an impetus to experimental and physiological psychology. How fitting, then, that there should be such a well equipped laboratory at Göttingen, realizing in a sense the long cherished hopes of the man who stood peerless among his contemporaries in the demand for thorough-going investigation and the application of the scientific method to all classes of facts.

The following is a descriptive list of the chief pieces of apparatus in the laboratory, arranged according to the kinds of experiment to which they are devoted.

I. *Psychometric and Reaction Time Researches.*

Hipp chronoscope (new pattern) with control hammer, the latter made by Krille. The whole construction embraces many new and ingenious improvements; also a device for the electro-magnetic

tuning fork with double slides and a micrometer screw for adjusting the same. Cost 140 marks.

Electro-Magnetic Signal Hammer. The head of the hammer is kept in equilibrium, not by means of the usual coiled-spring, but by means of a weight so arranged as to drop on a felt pad. This arrangement insures steadiness of the hammer and excludes all noise except the signal. Cost 25 marks.

Sprechcontactapparat (Schalltrichter) with the electro-magnetic contacts necessary to its best use. Cost complete 105 marks.

Ewald's double contact key with attachments, 31.50 marks.

Electrical keys and contacts; two compound contacts (five fold), simple keys and contacts, commentators, etc.

II. *Apparatus for Experiments upon the Time-sense (Zeitsinn)*, including platinum contacts and aluminium wheels. Pneumograph with tambour and attachments. Divers contact circles, a brass circular scale marked off into degrees, minutes and seconds, telephone statives, etc.

For other important apparatus in this line of research, see Schumann's exhaustive study in a recent number of the *Zeitschrift für Psychologie*.

III. *Researches upon the Memory.*

An especially constructed rotation apparatus, very complex in its arrangement.

IV. *For Psycho-physical Investigations.*

A set of eight weight holders, nach Fechner, together with weights (sheets of lead properly cut) belonging thereto. These are constructed after the manner of the originals used by Fechner himself in his experiments with lifted weights.

An arrangement (especially constructed and very ingenious) with aluminium levers for the purpose of recording the curves in experiments with lifted weights.

Apparatus for use in "Fühlraum" investigations, including an arrangement whereby the judgments are recorded in the form of curves drawn upon smoked paper.

Simple "Temperatursinnmesser" Warmtaster [Mischer].

Rotation discs with electrical apparatus for recording the clicks. A large "Rotationapparat" with five adjacent color discs, nach Aubert (Cf. Müller, Psycho-Physik). Licht-emdruck nach Exner (Wiener Ber. Nat. Hist., 2 Abtheilung 1868 Band 58 s. 60 ff.) for investigations upon the harmonizing of visual sensations; 280 marks.

An apparatus for fixing the head and holding it firmly, nach Helmholz. Darkened boxes used in the researches which have to do with light and color, Charpentier's "Kugeln," resonators, etc.

V. *Researches Along Various other Lines.*

Tonometer nach Mono, 180 marks. Spannungsmesser nach Bernstein, 125 marks. Kymographion with continuous paper.

Inducktorium, nach Du Bois-Reymond. Electrodes with clasps for the arms. Electrodes with arm bands, and various other electrodes.

VI. *Miscellaneous Apparatus.*

Resistance boxes (Siemens). Galvanometers with spring, nach Kohlrausch. Tuning fork interrupter, nach Helmholz, with spiral contact. Mariottes bottles and attachments. Water motor. Metronome Rheocord, a fine balance, and various other pieces of minor

importance. This by no means includes the complete equipment. All of the older apparatus which Müller used in the larger number of experiments referred to in his "Psycho-Physik," and in his more recently published magazine articles, are included and should not be forgotten, especially those concerned with his experiments upon sensations of movement and the muscle sense.

UNIVERSITY COLLEGE, CARDIFF.
Oct. 12, 1892.

My Dear Sir:

The plan of philosophical teaching in this college is to a large extent determined by the requirement of the University of London, which most of the students look to for degrees. The same is true of most of the provincial colleges of England and Wales as well as of University College, London. In our case, however, the advent of a University of Wales will—when it comes—no doubt introduce some changes into the curriculum; especially, it is to be hoped, in the direction of requiring more thorough knowledge of some special department of philosophy from candidates for the higher or M. A. degree.

As matters are arranged at present, I have courses of lectures on psychology, logic and ethics continuing throughout the session of thirty-two weeks. Lectures on psychology are given three times, on logic twice and on ethics once a week. In this way an outline of the three sciences is given; the London syllabus for the B. A. and B. Sc. degrees being kept in view and, to a large extent, followed. Some weeks at the end of the psychology course are, however, commonly saved for the purpose of introducing students to some of the philosophical questions which arise out of psychological questions. In the treatment of psychology, emphasis is laid upon the continuity of mental life. The physiological material admitted is comparatively small in amount; and it is only introduced in so far as it clearly helps to psychological conclusions. In this respect the method approaches what Professor Bain would call "ascetic." So far as my experience goes the method is fitted both to stimulate interest and to guard against the confusion of mental with material facts in which beginners are so apt to get entangled. In the class of ethics, a considerable portion of the time is occupied in explaining and commenting on one or two leading works by English moralists, such as Butler's Sermons on Human Nature and Mills' Utilitarianism.

In addition to these classes, advanced courses of lectures are given on the history of philosophy, with reference to special periods and books which change from year to year, and on the more difficult questions of logic, psychology and ethics, as well as on political philosophy. Both in the ordinary and in the advanced classes, the instruction by means of lectures is supplemented by conversation in the class-room by means of written papers.

I am, my dear sir,
Yours faithfully,

Prof. G. Stanley Hall. W. R. SORLEY.